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Vol. 218 — OCTOBER, 1934 — No. 4

CONTENTS

The Approach to Chemical Research Based on a Specific Example. CHARLES M. A. STINE 397

Television by Electron Image Scanning ..... PHILO TAYLOR FARNSWORTH 411

The Magneto-Optic Method of Chemical Analysis ..... FRANCIS G. SLACK 445

Emission of Electrons from Cold Metal Surfaces ..... CARL C. CHAMBERS 463

Housing Conditions in Relation to Scientific Machine Production ... HENRY WRIGHT 485

Notes from the U. S. Bureau of Standards 499

The Franklin Institute Notes 509

Book Reviews 511

Publications Received 514

Current Topics 498, 516

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Journal
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Devoted to Science and the Mechanic Arts

Vol. 218 OCTOBER, 1934 No. 4

THE APPROACH TO CHEMICAL RESEARCH BASED ON A SPECIFIC EXAMPLE.[1]

BY

CHARLES M. A. STINE, D.Sc., Ph.D., LL.D.,
Vice President, E. I. du Pont de Nemours and Company.

The theory, or method, of chemical research which I wish to discuss with you this evening interests me a great deal and seems to me to be deserving of more attention than the chemist engaged in synthetic organic research ordinarily accords it. Organic chemical research has long recognized the indispensability of the analytical approach. We painstakingly take apart natural products in order to learn their constitution and their structure. Sometimes this process of taking natural products apart is an exceedingly complicated and time-consuming one. Many years have been required to learn the constitution of scores of natural products such as the sugars, the dyestuffs, some of the proteins and a few of the resins. Even at best we must admit that the more complex of these organic compounds which we encounter in nature still demand better elucidation, since our knowledge of their structure is quite sketchy. If we knew more about the actual *structure* of the compounds which we wish to imitate, we would be less dependent upon slavish *imitation of chemical constitution* in our effort to duplicate the desired useful properties in the finished product. It is my purpose this evening, by means of a specific example—the synthesis of a new rubber—to contrast these two methods of approach, namely, the

[1] Cyrus Fogg Brackett Lecture at Princeton University. Subsequently delivered before The Franklin Institute on April 12, 1934.

(Note.—The Franklin Institute is not responsible for the statements and opinions advanced by contributors to the JOURNAL.)

method based on recognition of the characteristic, determining linkages or molecular arrangements in the structure, with the method based on the qualitative and quantitative duplication of the chemical elements present in the product. In the course of the discussion, and I hope without too great a digression, I wish to make a few collateral observations or suggestions, based upon this research, which have appeared interesting to me and provocative of thought.

It is probable that one of the least understood of all the chemical or physico-chemical reactions is that which we term "polymerization." It, no doubt, plays a decisive part in determining the physical properties, not only of resins, but is of primary importance in the processes of living organisms. The formation of proteins by polymerization of the amino acids is an interesting and striking example in the animal world which is familiar to all, whether chemist or non-chemist. It is more than possible that some of the diseases to which the human body is heir are due to polymerization "gone wrong." To cite another familiar example, it is certain that polymerization plays an exceedingly important part in nature's production of cellulose. It is difficult for us to imagine what a world would be like without cellulose, or any of the products derived from this wonderful material.

The specific case which I wish to use as illustrating a method of approach to chemical research is that of the synthesis of a rubber-like material from beta-chlor-butadiene, which was accomplished in the laboratories of the Du Pont Company several years ago. In the Journal of Industrial and Engineering Chemistry for November, 1933, Whitby and Katz, of the National Research Council of Canada, discuss the development of synthetic rubber. I wish that you had freshly in mind the historical portion of this article. May I quote, very briefly, a few sentences from their paper:

"The history of synthetic rubber can be conveniently treated under four heads: the early period, pre-war efforts to develop commercially applicable methods of making synthetic rubber, the actual manufacture of synthetic rubber in Germany during the World War, and post-war developments. Following the war there was a lull in the field of synthetic rubber research. The rise in the price of natural rubber

which took place in the latter half of 1925, however, served as a stimulus to renewed research. About this time a number of the larger companies in the United States engaged in programs of research on synthetic rubber, but, since the price of natural rubber dropped in 1926 and remained low, most or all of these companies abandoned this field of research, and only a few patents or other publications have issued as a result of these efforts. Two chemical manufacturing companies have, however, been more persistent in their efforts. The I.G. Farbenindustrie has been very active, and since 1929 a rapid stream of patents on synthetic rubber bearing its name has come forth. The post-war researches of the I.G.'s chemists, while not on lines fundamentally different from certain of the investigations pursued prior to the war, have developed substantial improvements in the production of synthetic rubber, especially in regard to the conduct of the polymerization process. The other company (du Pont) has approached the problem of producing synthetic rubber on distinctly novel lines, and its results, publications on which first appeared only about eighteen months ago, have an arresting character which has reawakened general interest in the subject of synthetic rubber. Starting with a study of polymers of acetylene, first prepared by J. A. Nieuwland, a group of du Pont chemists headed by W. H. Carothers discovered that 2-chlorobutadiene, obtained by the addition of hydrogen chloride to vinylacetylene, readily gives polymers, which, as good luck has it, are strikingly similar in elastic properties and, in certain respects, even superior to natural rubber."

There is one phrase in the above quotation which I desire to somewhat modify rather than accept it as printed. It is the statement that "2-chlorobutadiene, obtained by the addition of hydrogen chloride to vinylacetylene readily gives polymers, which, *as good luck has it*, are strikingly similar in elastic properties and, in certain respects, even superior to natural rubber." This was not quite entirely a matter of "good luck." The interesting history of this development of a new synthetic rubber is, in mere chronological and sequential outline, as follows:

Dr. Elmer K. Bolton, who is now Chemical Director of the

du Pont Company, was for many years in charge of the chemical research work of the Dyestuffs Section of that company, involving chemical research work on dyes, intermediates, rubber chemicals, and other organic compounds more or less closely related. It occurred to him in 1925 to suggest that we investigate a possible method of approach to the synthesis of rubber, which involved the oxidation of acetylene to di-acetylene, followed by its reduction to butadiene. This process, which had not been described in the literature at that time and appeared to be new from a patent standpoint, involved two steps from acetylene, whereas the Bayer process, operated in Germany during the war, involved a total of five steps from acetylene to dimethylbutadiene. Under the Stevenson plan, the price of rubber prior to this time had risen to as high as $1.05 per pound. Therefore, synthetic rubber looked like an attractive possibility.

In December, 1925, Dr. Bolton attended the first organic symposium of the American Chemical Society at Rochester, New York, and at one of the sessions of this meeting, Dr. Nieuwland presented a review of acetylene reactions in which he outlined the process of passing acetylene over cuprous ammonium chlorides to form divinylacetylene. Now, in Dr. Bolton's mind the critical importance of a conjugated double bond in the structure of the organic molecule of a certain type was the primary consideration which prompted his first suggestion which I have just described, i.e., the manufacture of butadiene through di-acetylene. It occurred to Dr. Bolton when listening to Dr. Nieuwland's brief description of divinylacetylene, in which Nieuwland called attention particularly to the ability of pure divinylacetylene to form explosive compounds which would detonate with violence, that if it were possible to prepare monovinylacetylene in good yield it would afford an interesting starting point for this research work on synthetic rubber. Dr. Bolton accordingly discussed this question with Dr. Nieuwland and learned that Dr. Nieuwland felt that it might be possible to make monovinylacetylene, as he suspected the formation of small amounts of the material along with the divinylacetylene. It seemed to Dr. Bolton that monovinylacetylene would be of

particular interest because it offered the possibility, by reduction, of giving butadiene, which we were at that time investigating and attempting to prepare from di-acetylene. Arrangements were made with Dr. Nieuwland to undertake the study of Nieuwland's synthesis of vinylacetylene in our research laboratories. Most of our earlier work was concerned, as I have said, with making di-acetylene, and trying to reduce it to butadiene.

Because there appeared in 1926 in the *Berichte*, an article by Strauss and Kollek on di-acetylene, which covered substantially the process which we were carrying out of making it from acetylene, we concentrated our attention on the possibilities involved in using Nieuwland's synthesis of vinylacetylene as a starting point. The German investigators had found it very difficult to get good yields of di-acetylene and we concluded that it probably would not be an economical process for the preparation of butadiene.

I might point out in this connection that we also carried out some research work on the reaction between ethylene and acetylene to form butadiene. This reaction had been described in the German patent literature. According to our results, however, the percentage of butadiene formed was so small as compared with the amount of ethylene polymerized to other high boiling compounds, that we did not at that time further pursue that section of this research.

The research work which concerned itself with Nieuwland's synthesis of divinylacetylene and monovinylacetylene was earnestly prosecuted over the ensuing years—1926, 1927 and 1928. A good deal of this work was concerned with the effort to make rubber-like materials from divinylacetylene. In this connection it was found possible to prepare from divinylacetylene, materials which polymerized very rapidly.

I well remember one winter afternoon, when I was still Chemical Director of the du Pont Company, Dr. Bolton still being in charge of the research work in the Dye Department, that he asked me to spend some time with him and the group of men engaged in this research work in order to examine divinylacetylene from the standpoint of its suitability as a starting point for synthetic resins which might prove interesting as protective coatings. After a good deal of work

with divinylacetylene, we decided that divinylacetylene would be much more interesting as a synthetic drying oil which would polymerize to a synthetic resin. The synthetic drying oil produced from divinylacetylene has the interesting properties of polymerizing when exposed to the air to a dry film which is exceedingly inert toward chemical reagents. I might say in passing that the pigmentation of this film, the study of improved adhesion, especially on metal surfaces, and the elimination of a tendency to spontaneously decompose, in the case of clear varnishes, under special circumstances, are all problems the successful solution of which has engaged the attention of our research chemists.

Although we had found it possible to produce resinous polymers of divinylacetylene that possessed very interesting properties we were unsuccessful in our efforts to produce from it the rubber-like polymers that were the original and primary objective of our work. This was not particularly surprising in view of the fact that divinylacetylene did not contain the two conjugated double bonds that were recognized as the essential feature of materials capable of polymerizing to a rubber-like product. At that point we directed our efforts particularly to monovinylacetylene which contained a double bond conjugated with a triple bond. I have previously spoken of Dr. Bolton's thought that monovinylacetylene might be of interest because of the possibility of producing butadiene from it by hydrogenation but butadiene did not appear to be a satisfactory starting point for synthetic rubber because it polymerizes even more slowly than isoprene. It then occurred to those engaged in this work that hydrogen was not the only material with which the triple bond of monovinylacetylene could be reduced to a double bond since it appeared probable that a synthetic rubber might be produced from any compound having the conjugated double bond structure regardless of the nature of the substituents on the carbon atoms. In the light of all that was then known of synthetic rubbers this was a bold assumption indeed. With this assumption in mind, further work on the reactions of monovinylacetylene was carried out which resulted in the discovery of the fact that hydrogen chloride is readily added in the presence of a suitable catalyst.

That the chloro-butadiene which resulted from the addition of hydrogen chloride to monovinylacetylene polymerized to form a rubber-like material surprised none of those who were engaged in this work. Such a result was to be expected from our knowledge of the critical importance of the conjugated double bond. What did surprise us, however, was the exceedingly great influence of the chlorine atom on the rate of polymerization. The products of the reaction of hydrochloric acid with monovinylacetylene had been carefully purified by distillation in a high vacuum and a certain amount of the pure material had been allowed to stand all night in a glass receiver. We were highly delighted, but not exceedingly surprised to find that this material had set up to an elastic mass, closely resembling natural rubber. Perhaps the thing that really surprised us in this result was the speed with which polymerization occurred under certain conditions. You well know that the various experiments on the syntheses of rubber, from butadiene and its homologs, have been characterized by exceedingly slow polymerizations of the dienes. 2-chlorobutadiene polymerizes very rapidly—several hundred times as fast as butadiene itself or methyl butadiene (isoprene)—to the rubber-like material which we christened "Du Prene."

When Greville Williams in 1860 first isolated isoprene and recognized natural rubber to be a polymer of isoprene he had no means of knowing whether the methyl group on the second carbon atom was an essential or an unessential feature of the compound. His position was almost analogous to that of a native tribesman of the African Congo, who upon first sight of an automobile might be uncertain as to whether the motor under the hood or the ornament on the radiator filler cap is the thing that makes it go. Today we know that the conjugated double bond is the essential feature of isoprene, so far as its capability of polymerizing to a rubber-like structure is concerned. The methyl group is a mere frill, like the radiator filler cap ornament. Before we had this knowledge the only way of approaching the synthetic rubber problem was to attempt to produce isoprene itself in the laboratory but now that we not only know the structure but are able to analyze it functionally we have produced a synthetic rubber in which the non-essential methyl group has been discarded

and replaced by the substituent chlorine that really contributes to the usefulness of the compound.

Examples analogous to this one might be taken from many other branches of chemical research. You are all familiar with the great progress that has been made in medicinal chemistry since we began to recognize the functions of the structural components of various drugs. We are beginning to have a clearer understanding of the macro-molecules produced by polymerization but before we can make real progress in the synthesis of these compounds of large molecular weight we must know not only what they are but why. Without that knowledge we shall indeed be dependent upon the favor of the goddess of luck.

The study of the methods of controlling the rate and character of the polymerization of 2-chlorobutadiene has proved not only time consuming, but exceedingly interesting. I suppose no one is very well versed in what I might term three-dimensional chemistry. Since polymerization is an outstanding example of three-dimensional chemistry, as well as a conspicuous example of a neglected field in the science, the first thing that impresses the investigator is the paucity of reliable research results dealing with polymerization. It is, therefore, not remarkable that we have found the elucidation of the mechanism of polymerization of 2-chlorobutadiene, and the development of various methods of directing this polymerization along desired channels, an expensive and time consuming investigation. You will, I am sure, therefore, rejoice with me in the happy outcome of this work.

While at first we found it difficult to duplicate results on a plant scale, in that successive batches of this synthetic rubber did not exhibit identical properties when milled and processed, we have steadily improved the homogeneity of the products and our ability to duplicate results. This synthetic rubber, which I will refer to as "Du Prene" in order to facilitate the discussion, exhibits many interesting properties. I have no doubt that you are familiar with many of them, since they have all been subjects of discussion in the technical literature, and I shall merely refer to them in passing.[2] The

[2] *J. A. C. S.*, **53**: 4197, 4203 (1931); **54**: 4066, 4071 (1932); **55**: 1624, 1628 (1933). *J. S. C. I.*, **52**: 416 (1933). *Ind. & Eng. Chem.*, **25**: 280, 1219 (1933); **26**: 33, 307 (1934); News Ed., **9**: No. 21.

production of a rubber which resembles vulcanized rubber, but which does not require the use of sulphur in the production of a cured product from Du Prene, is perhaps the most striking difference between Du Prene and isoprene rubber. It is to be expected that the presence of a large per cent. of chlorine —40 per cent.—in the molecule would be responsible for certain alterations in the physical and chemical properties of the resulting polymer. Resistance to the solvent action of gasoline and oil is another one of the outstanding differences between Du Prene and natural rubber. Resistance to decomposition when exposed to ozone produced by corona discharge is yet another outstanding difference between Du Prene and natural rubber. The tensile strength, elongation and other physical properties of various types of Du Prene polymers containing various ingredients such as carbon black, magnesium carbonate, zinc oxide, and the like, are a matter of record in the technical literature and their repetition does not interest us in this discussion.

I come now to the heart of the matter—that is, to the theory—which interests me particularly. Had we attempted to adhere to our original plan of approaching the synthesis of rubber solely by duplicating, as nearly as we could, the chemical structure of natural rubber, we should perhaps be no further along with this problem today than we were eight or nine years ago. The recognition that a type of chemical structure such as we know as a conjugated double bond, might give rise to desired physical properties in an end product *essentially different in chemical constitution from the natural product* was the essential element in this unusually interesting and successful piece of chemical research. Permit me to dwell upon this for a moment, for it appears to me to be of far reaching significance. Chloro-2-butadiene-1,3 by calculation should contain 54.25 per cent. carbon, 5.69 per cent. hydrogen and 40.06 per cent. chlorine. The molecular weight of the polymerized product must, of course, be very high, but the molecular weight of the colorless liquid, which boils at 59.4° at 760 millimeters, is 89.0 in freezing benzene (88.5 theoretical). On the other hand, the proximate analysis of butadiene shows about 88.9 per cent. carbon and 11.1 per cent. hydrogen, with a molecular weight of 54 (theoretical),

and the proximate analysis of 2 methyl-1-3 butadiene shows about 89.6 per cent. carbon and 10.4 per cent. hydrogen, with a molecular weight of 67 (theoretical).

In other words, when examining the constitution of these molecules qualitatively, and comparing the compounds purely from the standpoint of the analytical chemist, the widest differences exist between chloroprene and isoprene, but when the molecules are examined structurally, very close and striking analogies at once present themselves. If we had the necessary knowledge and tools available for studying the physical structure of the finished polymers (Du Prene and natural rubber), I am sure that not only striking resemblances, but equally interesting and striking differences would be encountered. Striking differences must exist in the structure of the finished polymer, first, because of the presence of chlorine in Du Prene in the place of the methyl group in isoprene, and secondly, because of the differences in physical properties and the contrasting behavior in certain respects which we note when we compare polymerized chloroprene with polymerized isoprene.

Another very interesting observation which has been of critical importance in connection with the study of polymerization of chloroprene to Du Prene has been the effect upon the rate of polymerization produced by varying amounts of, and varying times of exposure to, oxygen. To put it a little differently, oxygen concentration, time and temperature are all of critical importance in determining the rate of polymerization and the type of polymer produced. The effect of oxygen compounds and of organic peroxides upon the polymerization of various types of organic compounds has been noted in various connections. For example, we are familiar with the very important bearing upon the rate of drying of certain vegetable oils such as tung oil and linseed oil, of the presence of combined oxygen, either in the form of driers such as cobalt linoleate, or of organic peroxides. There is very good ground, therefore, for believing that organic peroxides are of critical importance in determining the rate and nature of polymerization of organic compounds in certain classes of polymers.

We have found, for example, that reducing agents which

presumably function by preventing formation of oxygen compounds, will act as polymerization inhibitors in some instances; in other instances will completely alter the type of polymerization. We know that exposure to light is also of critical importance in connection with certain types of polymerization. For example, by strongly illuminating chloroprene we can induce rapid polymerization and affect the nature of the resulting polymer. There has been plenty of evidence for the belief that light, heat and oxygen all affect the rate and type of polymerization in a number of types of organic polymers.

All this leads me to feel that if a good deal more emphasis were placed upon three-dimensional chemistry some wonderfully interesting results might be forthcoming. Perhaps the physicist, the physical chemist and the organic chemist would constitute a triumvirate which would make a successful attack upon the problem of the actual structure of many highly interesting and important products. If, for example, we were able to determine exactly the constitution of a bristle we might not, then, be limited to hog bristles for our most satisfactory commercial product. Furthermore, it is more than likely that the exact chemical elements in the natural product and the proximate analysis for carbon, hydrogen, nitrogen, sulphur and the like would no longer constitute such important information in the mind of the chemist embarked upon the task of the synthetic development of a material to duplicate the physical properties of a specific natural product. I fully realize, of course, that the problems involved in three-dimensional chemical research are many and complex. We lack tools—both physical apparatus and reactions—to attack this problem. The very difficulty of the task should appeal to some of our outstanding research workers who may possess the divine spark of genius. I have for a long time believed that the approach to the development of synthetic materials, intended to duplicate the useful working properties of some of the natural products largely used by man, neglected and, in too many instances, completely ignored, the possibility of producing the desired material from chemical elements radically different from those found in the natural product.

Max Bergman's work on the structure of various types of amino acids with its bearing upon the structure of proteins, appears to be an important step in the direction of the control of polymerization. As soon as we are able to polymerize or depolymerize a step at a time, this step-wise control will take us a long way forward on the route to the elucidation of polymerization and the control of the physical properties of the resulting polymers. If we only understood the physical structure, the method of assembling—building together—the molecules into the fiber, or the resin, or the protein, what marvelous advances might be made in developing wholly new products! New products of this nature might in some instances exhibit quite surprising variations from the natural products which, in other properties, they would closely resemble. What an exciting pursuit, indeed, this method of research might prove to be to those with the mental equipment and the training necessary to undertake this somewhat more complex type of synthesis!

Growing out of the attention to the phenomena of polymerization, another line of thought which has greatly interested me is the possible bearing of the rate and type of polymerization occurring in the human metabolism upon the normal functioning of the human body, or, to put it in chemical language, its bearing upon the usual course of the chemical reactions which constitute the vital processes. Dr. G. Arbor Stefans last September published an article entitled "Further Observations Regarding Carbon Monoxide as an Important Factor in the Causation of Industrial Cancer." I quote a few of the significant statements from this article:

"Anoxæmia, or a shortage of oxygen, is of frequent occurrence. The coefficient of oxygen is the amount of oxygen used by an organ per gramme per minute. . . .

"A good estimate of this basal metabolic rate can be made by taking the basic blood-pressure, described in my book 'The Heart and Spleen in Health and Disease.' When this basic blood pressure is raised, oxidation is disturbed. Age, starvation, fever, cold, consumption of meat and activity of the ductless glands disturbs both the basal metabolism and the basic blood-pressure.

"There are three kinds of anoxæmia: (1) The anæmic; (2) the anoxic; (3) the dormant. In the anæmic there is less

hæmoglobin to carry the oxygen. The anoxic occurs in high altitudes and the dormant occurs in the sluggish flow of blood through the heart muscles. In anoxæmic patients the effects of exercise are marked. Respiration is quickened, heart beats quickly and digestion fails. In these days of motoring the carbon monoxide fumes tend to produce anæmic conditions, because CO has a stronger affinity for hæmoglobin than oxygen has, and so the latter is driven away. . . .

"Carbon monoxide acts solely by displacing oxygen from its combination with hæmoglobin, forming a stable compound which is useless for oxygen-carrying purposes. The affinity of CO for hæmoglobin is about 300 times as great as that of oxygen. If the normal 14 per cent. oxygen be present in the alveoli; and there be 0.047 per cent. CO, the hæmoglobin of the blood shares itself out equally between the two gases. Minute amounts of CO, if constantly maintained in the alveoli, can thus produce serious results.

"The reason why the symptoms are so severe when 50 per cent. of the hæmoglobin is not functioning, and why so mild when the hæmoglobin is reduced to the same amount, as the result of anæmia, is because the presence of COHb alters the dissociation curve of the functioning hæmoglobin which is still present. . . .

"There is one form of anæmia that has hitherto received very little attention. I refer to senescence anæmia, or that which occurs in later life. For some reason or other the evolution of the blood cells has not synchronized accurately with the age of the patient, and there is a lack of correlation between the blood cells and those of the body tissues. This senescence anæmia is one which makes the patient particularly liable to poisoning even by small doses of CO gas, with the result that the amount of oxygen carried along by the blood is reduced in quantity. The natural consequence is that the tissue cells are so devitalized as to allow of the ready formation, or preferably transformation (as described in my book, in the chapter on 'The Heart and Blood, and the Virus Problem'), of the colloidal 'cancer' virus. The modification of the colloidal elements in or near the several cells is a subject of very great importance. Colloidal changes are so often due to the presence of catalysts; but unless the conditions are congenial the catalysts are unable to act. In any case the

liability of men with senescence anæmia is so great that men over fifty-five ought not to be allowed to work in rooms where CO is constantly present even in small quantities."—*The Medical Press and Circular*, page 283 et seq.

The effect of very small amounts of carbon monoxide, combined with the hæmoglobin of the blood, as determining the oxygen pressure or oxygen concentration in the blood, is profound as indicated by these observations. I am led to speculate as to the effect of even small differences in oxygen partial pressure in the blood upon the possible formation of minute amounts of organic peroxides. Since oxygen and organic peroxides are very powerful catalysts for inducing polymerization in certain types of reactions, may it not be possible that the partial pressure of oxygen in certain portions of the human body may really be of critical importance in determining the rate and type of polymerization, or depolymerization, which may be involved in some of the chemical reactions of the living, growing, organism, and that oxygen may, therefore, play an exceedingly important part in a hitherto unsuspected fashion in determining health or inducing certain kinds of disease. While you may not agree with me in these speculations, I sincerely hope that some of you may be moved to investigate their correctness, possibly with consequent observations of great importance.

I am sure that if chemistry is to advance as it should, the study of polymerization, as one of the more important reactions in determining physical structure, must receive vastly more attention than it has hitherto. I am sure, further, that the elucidation of structure from the standpoint of the physicist as well as that of the chemist, and the development of much more knowledge in the field of three-dimensional chemistry will prove to be not only highly productive as an intellectual pursuit and as a contributor to our fund of knowledge of the structure of the world, but that it will prove also to be a highly remunerative study for the research worker who labors under the limitation, not only of having to discover new reactions which are interesting, but new reactions which are capable of yielding products which will add to man's comfort, health and enjoyment of the world about him.

TELEVISION BY ELECTRON IMAGE SCANNING.

BY

PHILO TAYLOR FARNSWORTH.

A television system is described, in which the complete optical image to be scanned is focused upon a sensitive photoelectric cathode surface, thus producing a corresponding "electron image" of photoelectrons at the surface. This electron image is brought to a sharp focus in a plane parallel to the cathode surface by means of uniform magnetic and electric fields, perpendicular to the surface. Scanning is accomplished by displacing the focused electron image with two mutually perpendicular magnetic fields (which are themselves perpendicular to the focusing field), and thus sweeping the electron image across a fixed aperture.

The current through the coils which produces the horizontal displacement of the electron image, has a saw-tooth wave form of constant frequency (line frequency) which is in the range of 5,000 to 10,000 repetitions per second. The current through the coil producing the vertical displacement has a saw-tooth wave-form of constant frequency (image frequency) which may be 16 to 30 repetitions per second. Means for producing these saw-tooth currents and means for synchronizing them between the "Image Dissector," or transmitting tube, and the "Oscillight," or receiving tube, are discussed.

A new method of amplifying very feeble electric currents by making use of secondary electron emission is described. This method of "Secondary Electron Multiplication" makes use of cold cathodes which have a high ratio (6 or greater) of secondary electrons to primary impacting electrons and uses this effect to produce an increase in an electrical current. Means are described for making use of the principle of secondary electron multiplication for producing amplification of photoelectric currents (amplification greater than 5×10^7), and of feeble directed electron streams (amplification greater than 10^3), and for the production of radio frequency oscillations (20 megacycles or greater).

The image dissector tube used without an electron multiplier requires a light intensity of 15 to 25 lumens to bring the signal impulse above the noise level inherent in the amplifier circuit and is thus suitable for use as a pick-up from motion picture film. By introducing an electron multiplier tube within the structure of the image dissector tube, placing it behind the image receiving aperture, the currents within the tube are amplified more than 10^3 times without appreciably increasing the noise level of the tube, and the tube is made sufficiently sensitive to pick up the image from a directly illuminated subject or from outdoor scenes under moderate daylight conditions.

The receiving tube or "Oscillight" consists of a cathode ray tube whose gun delivers a beam of 1 to 15 milli-amps. within an angle of 15 degrees. This beam is focused on the fluorescent screen by means of a short magnetic solenoid around the neck of the tube near the gun and coaxial with it. The horizontal (line frequency) scanning is produced by magnetic deflection coils which are at right angles to the

axis of the electron gun and carry currents of the same waveform and frequency as the corresponding current in the image dissector tube. The low frequency, or picture frequency, scanning is produced by a magnetic field normal to the line frequency displacement field. This low frequency field makes use of core pole pieces of special shape.

1. ELECTRON IMAGE—DEFINITION OF THE TERM.

The fundamental idea underlying electron image scanning [1] is to receive from the optical image an electronic discharge in which each portion of the cross-section of such electronic discharge will correspond in intensity with the intensity of the light incident on that portion of the sensitive plate from which the electronic discharge originated. Then, if the electron image is bombarded against a fluorescent screen, the optical image will be reproduced there. Such a discharge is termed an "electron image."

Scanning of the electron image is accomplished by deflecting the entire electron discharge in such a manner that each elementary portion is successively picked up by a minute aperture in a suitable collector.

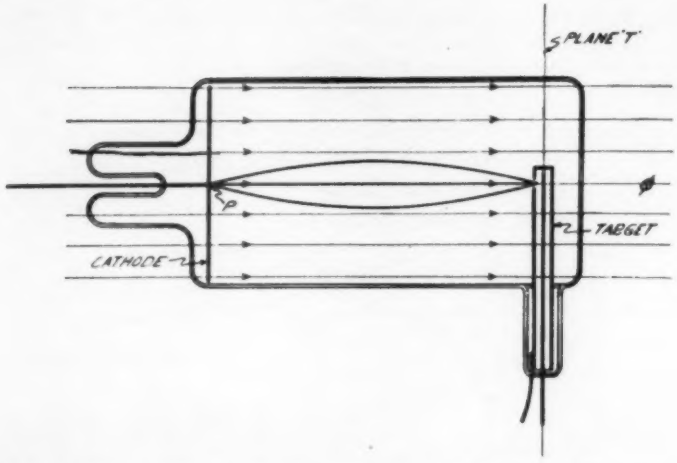
2. CONVERSION OF OPTICAL IMAGE TO AN ELECTRON IMAGE.

An optical image focused upon a uniform photoelectric surface produces an electron emission from each point proportional to the intensity of the light at that point. If all of the electrons emitted from the surface are drawn away from it, such a discharge is classifiable as an electron image. The image, however, is sharp only in the plane of the cathode.

In order that the electron image may be scanned by deflecting it over a small aperture, the correspondence to the optical image must be present at a suitable distance from the cathode. There are several possible methods for fulfilling this requirement. Referring to Fig. 1, if all of the electrons leaving point "*P*" on a cathode, could be constrained to travel parallel to one another, then any transverse section of the beam would represent a perfect electron image. For all electron paths to be parallel there must be no component of transverse velocity. The photoelectrons, however, are emitted with random velocities, ranging from zero to a volt or more. Another

[1] See Farnsworth's U. S. Patent No. 1,773,980.

FIG. 1.



Envelope of focused electron paths from one point on the image in an image dissector tube.

component of transverse velocity is added by the non-uniformity of the electrostatic lines of force, and another large component by the microscopic irregularity of the cathode surface. Thus, the photoelectrons leave each point on the photoelectric surface as a divergent cone. We can secure something of an electron image in the plane of the anode target T, by the use of low frequency light, careful construction of the cathode of the tube, and the use of high potential between the cathode and the anode. The dissector tube, in fact, was first used in this manner.

3. MAGNETIC FOCUSING AND DEFLECTION.

In the practical form of image dissector tube now in use, the electron image is focused magnetically. To accomplish this, a uniform magnetic field of exactly the proper intensity, ϕ, is adjusted so that the lines of force are parallel to the axis of the tube. The effect of this magnetic field is to bend the electrons into helical paths, tangent to the line of magnetic force through the emitting point. Neglecting, for the present, the longitudinal velocity (see Fig. 2), and considering the helices as circles in the plane of the point "P," we see that we

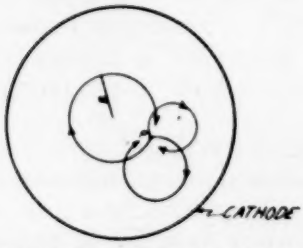
have a family of circles, all tangent to the point "P." The radius, r, of these circles is given by the relation:

$$r = mv/\phi e, \tag{1}$$

where

v = transverse velocity of electron,
ϕ = strength of magnetic field,
e = electron charge,
m = mass of electron.

FIG. 2.



Projection of helical electron paths in the plane of the cathode.

The angular velocity ω, with which an electron travels around the circle is given by:

$$\omega = v/r. \tag{2}$$

Substituting this value of r in equation 1, we have

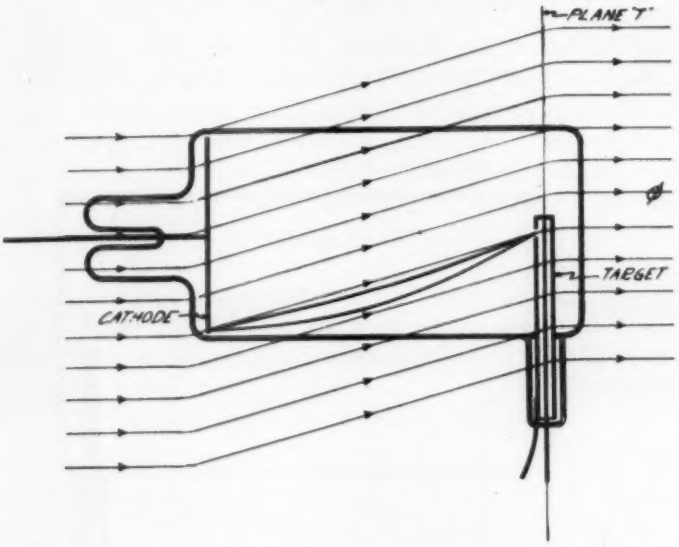
$$\omega = \phi e/m; \tag{3}$$

thus, it will be seen that the angular velocity is independent of the value of the transverse velocity of the electron. Therefore, since all of the electrons start at the same point, they will arrive back at the same point, when $\omega t = 2\pi$, that is, when they have traversed the circle once, or for any value of magnetic field intensity which gives an integral number of revolutions. In practice, the first focus is used.

The axes of the helices need not be within the electron pencil; if the longitudinal velocity is the same for all, every electron, no matter how divergent, will eventually return to intersect a line of magnetic force passing through its point of origin. It follows, therefore, that if the direction of the field be changed, the point at which the rays are focused will

follow the field. If, therefore, we superimpose a transverse magnetic field on the longitudinal field, the electrons will be deflected to follow the resultant field, as shown in Fig. 3.

FIG. 3.



Envelope of focused electron paths when a transverse component is added to the initial magnetic focusing field of the image dissector tube.

It will be seen, therefore, that a pair of coils placed one on each side of the dissector tube, and carrying an alternating current will give a deflection along the axis of the coils, that is, in the direction of the magnetic lines produced by these coils, rather than at right angles to this direction, as would be the case if there were no longitudinal or focusing field.

4. RESOLUTION OBTAINABLE IN THE ELECTRON IMAGE.

The degree of sharpness of an electron image focused magnetically is inversely proportional to the angle of the emergent cone filled by the unfocused electrons. The electron image may be sharper than the optical image produced by the best lens, by sufficiently limiting the angle of the emergent cone. In the dissector tube the electron image is much sharper than required. This result is achieved by using a cathode

that is perfectly smooth (almost polished), and an accelerating potential of approximately 700 volts. The sharpness of the electron image can be directly observed by bombarding it against a fluorescent screen.

5. CONSTRUCTION OF THE IMAGE DISSECTOR TUBE.

A practical form of image dissector tube suitable for scanning motion picture film is shown in Fig. 4. It comprises

FIG. 4.

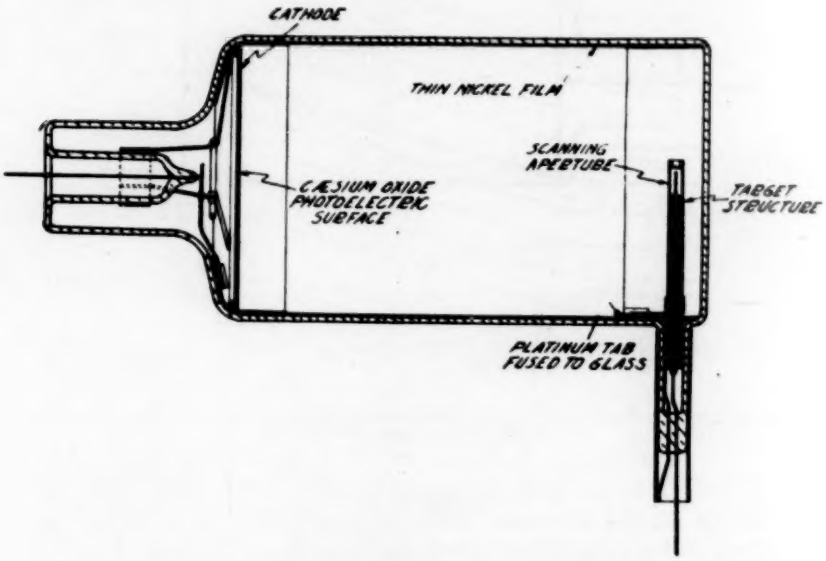


Image dissector tube used for scanning motion picture film.

an evacuated envelope in which is located a photoelectric cathode and a combination anode and target electrode. The walls of the tube are coated with a thin evaporated film of nickel. One end of this film is connected to the cathode and the other end to the anode. The electrical resistance of this metallic film may be anywhere from ½ megohm to several megohms. The cathode is of electrolytically pure silver. The surface must be cleaned and a uniform crystal structure, and preferably having a very fine, uniformly etched finish.

Sensitization of the cathode follows very closely the usual technique [2] for such silver oxide-Cæsium cathodes.

The photoelectric cathode of the dissector tube must be uniformly sensitive over the entire surface, since any variations will appear in the image as dark or bright areas. It has been found that uniformity in color over the surface usually indicates uniformity in sensitivity. To obtain such uniform color requires not only that the silver surface be of uniform texture and thoroughly clean, but also that the oxidization process be carried out uniformly. The color of the entire surface should be the same after it has been properly oxidized. It is also important not to form the surface too rapidly, and no Cæsium must be allowed to condense directly on the cathode.

The photoelectric sensitivity of such tubes has varied in the past between 10 and 30 micro-amps. per lumen. Recently this sensitivity has been increased to 50–65 micro-amps. per lumen.

6. DEFLECTION AND FOCUSING COIL SYSTEM.

In the dissector tube shown in Fig. 4, the electron image is focused by means of the solenoid wound around the outside of the tube. Good focusing requires that the current in this solenoid be adjusted very accurately and, of course, the accuracy of the focus required will depend directly on the size of the scanning aperture which is to be used. The size of the scanning aperture we regularly use is .015 inch, allowing approximately 240 lines, since the cathode surface is 4 inches in diameter. This requires that the focusing current be adjusted to approximately plus or minus 2 per cent. The number of ampere turns used in the focusing coil is, approximately, 500 for a difference in potential between cathode and anode of 700 volts.

Deflection of the electron image for scanning is accomplished by two sets of coils at right angles to the dissector tube, and each pair of coils must be mounted at right angles to each other. These coils are rectangular, designed somewhat on the basis of the coils in the D'Arsonval Galvanometer. A

[2] See for example, Prescott and Kelly, *Bell System Technical Journal*, Volume XI, July 1932, p. 334.

square deflection field is obtained by spacing these coils a distance apart, approximately equal to their shortest diameter. It is an advantage also that the coils extend the full length of the dissector tube. The number of turns in the coil may be adjusted to suit the particular generator that is to feed them, but using the generators we have developed, low frequency coils have 5,000 to 10,000 turns per coil. The high frequency coils, on the other hand, have about 65 turns per coil.

In mounting these coils on the tube, they must be mounted so that mutual inductance between pairs at right angles to each other is as low as can be obtained; otherwise, difficulty is experienced with one pair resonating to harmonics of the current in the other pair. This gives a wavy appearance to the scanning field that is very troublesome. To overcome this difficulty, the two sets of coils should have widely different impedances and should be adjusted for minimum mutual inductance as stated before.

7. SENSITIVITY OF THE DISSECTOR TUBE.

The sensitivity of the dissector tube when used with a 15,000 ohm coupling resistor is not adequate for scanning directly from the subject, and if it is used in the form shown in Fig. 4, very intense light is required for illuminating the subject. This type tube is used, therefore, only for motion picture work, and for such use a light source is used which gives 15 to 25 lumens in the optical image on the cathode. With this light intensity the average signal in the output of the dissector target across 15,000 ohms, is approximately 1/10 milli-volt.

When the dissector tube is to be used for direct scanning the electrons selected from the electrical image are first multiplied by a process called "electron multiplication." In this manner, the sensitivity of the tube is increased several thousand times.

8. SECONDARY ELECTRON MULTIPLICATION.

When an electron stream is directed against a surface, it causes the emission of secondary electrons. These secondary electrons may exceed in number, those of the original or primary beam, if the primary electrons have sufficient velocity, This fact may be utilized to amplify an electron flow. Thus,

an electron stream which is to be amplified is directed against a surface capable of emitting as many secondary electrons as possible by the impact of the primaries. The secondary electrons themselves are then directed against a similar surface. This process is repeated as many times as desired and the total electron flow finally collected.

9. CONSTANT POTENTIAL TYPE MULTIPLIER.

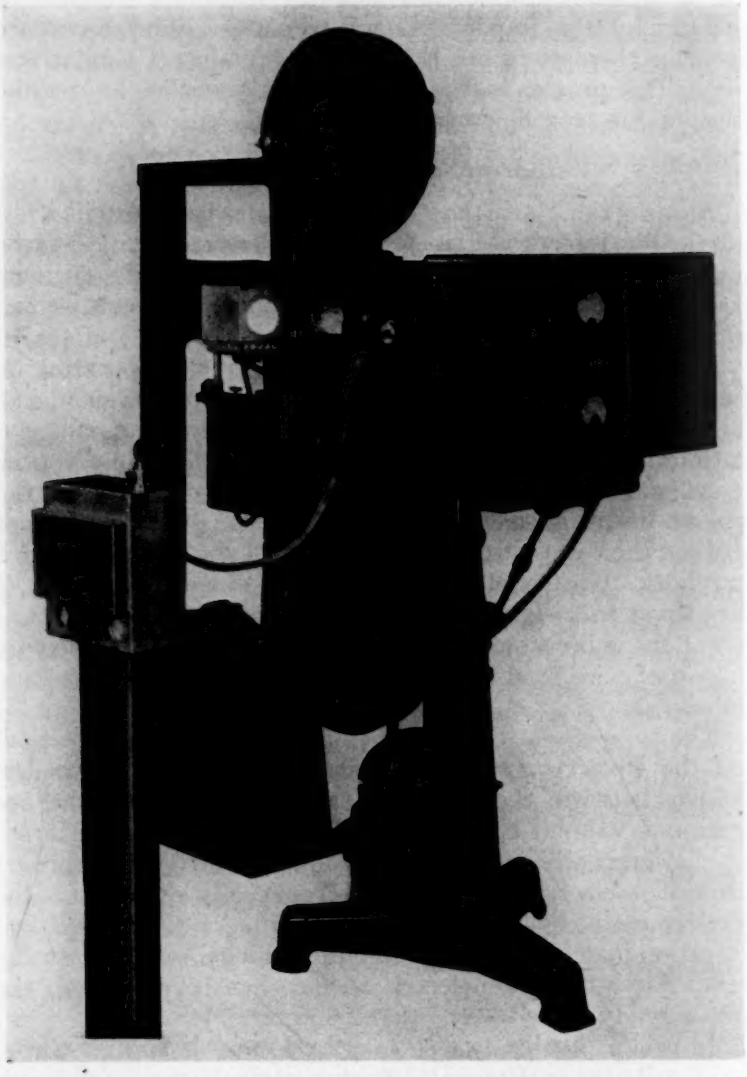
As an example of how this process may be carried out practically refer to Fig. 6. This represents an electron multiplying photocell in which the photoelectric currents from the upper end of the tube may be multiplied many times by successive secondary emission impacts.

The tube is constructed by sputtering or evaporating on the walls of a tubular bulb, a thin film of platinum or nickel, and making connection at each end of this film by means of a platinum tab fused to the glass. It is worthwhile, in addition, to evaporate a rim of heavier film around each end of the tube to insure good connection to the film. The resistance between the two platinum tabs is made of the order of 0.5 to 2 megohms. On top of this nickel film is evaporated a film of pure silver to a thickness that is nearly opaque. Down the axis of the tube is mounted an anode consisting of a fine wire or loop of wire, and having at one end a metallic collection plate as shown.

The tube is sealed onto the pump, pure oxygen admitted, and the silver surface completely oxidized so that the resistance between the platinum tabs again assumes the approximate value it had before the silver was evaporated onto it. Cæsium is then driven into the tube and the tube formed into a photoelectric cell. This process usually lowers the value of resistance somewhat, and this fact must be taken into consideration in deciding on the amount of nickel to be put on the wall. After the photoelectric surface has been formed, the tube is ready to be sealed off the pump.

In use, a potential of 500 volts or more is applied across the distributed resistance so that a potential gradient longitudinal to the tube results. At the same time the greatest positive potential in the tube is that of the anode. Photoelectrons emitted from the top of the tube are accelerated

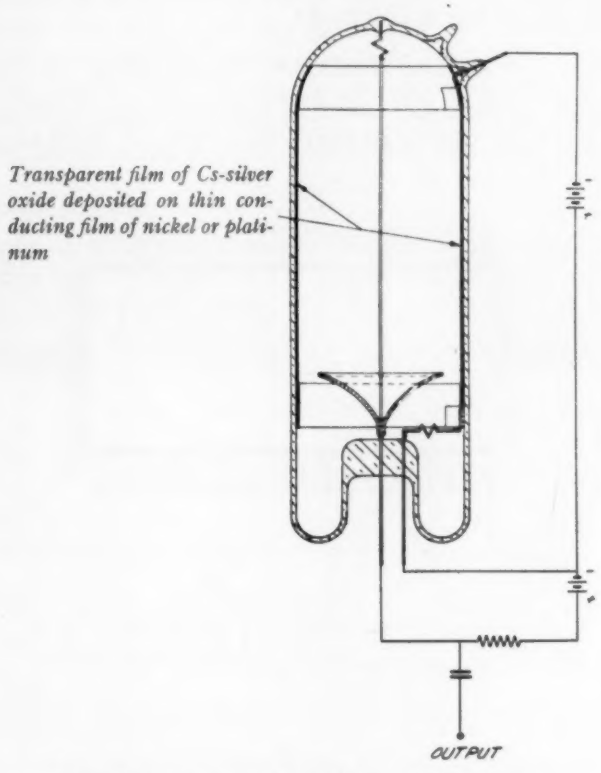
FIG. 5.



Transmitter unit used for scanning motion picture film. The image dissector tube and head amplifier are in the box on the right which is mounted on the projector unit. The line amplifier is separately mounted in the box at the left. The moving picture film is moved continuously past the picture aperture and no picture frequency deflector coil is used on the dissector. The impulses for synchronizing the picture frequency of the receiving system with the film are obtained from a special impulse generator connected to the film driving sprocket.

toward the anode but due to the gradient longitudinal of the tube they also fall down the tube by a definite amount and strike the wall opposite which they were emitted with a velocity that is proportional to the potential gradient through which they have fallen. This gradient may be anywhere

FIG. 6.



Photocell which makes use of the electron multiplier principle.

from 50 to 100 volts, or more. The secondary electrons knocked out by impact of this primary beam are accelerated across and down the tube in a similar manner; upon reaching the opposite surface, cause an emission of electrons in increased numbers. Thus, each transverse passage results in an increase of the number of electrons. With a good Cæsium-

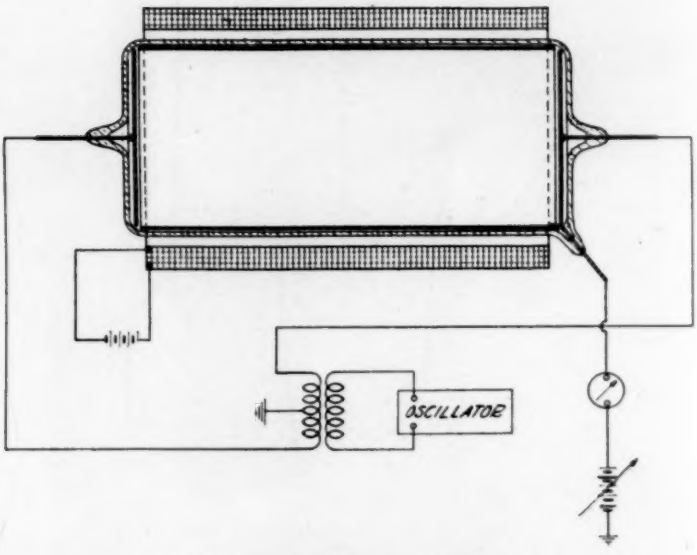
silver oxide surface this increase amounts to a factor of three to six times for each impact.

The amount of electron multiplication obtained by this method is a function of the average potential through which the electrons fall between impacts and of the number of impacts. Up to the point where the space charge effect supervenes, the final electron flow is directly proportional to the number of initial electrons.

10. RADIO FREQUENCY TYPE MULTIPLIER.

Figure 7 shows the essential features of an electron multiplier structure, that is all that is necessary to demonstrate the

FIG. 7.



Radio frequency type electron multiplier tube.

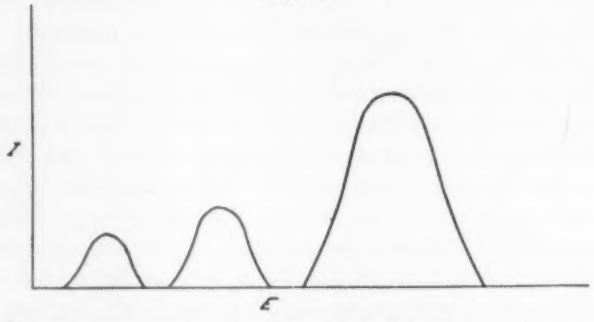
characteristics of another method of carrying out the multiplication process. As is shown in the figure, the tube comprises a cylindrical evacuated envelope having plate like cathodes mounted in each end. These cathode plates are preferably made from pure silver, oxidized and formed with Cæsium into sensitive secondary emitting surfaces. A cylindrical anode

of nickel or molybdenum is positioned between the cathode plates. This anode may fill almost the entire space between the cathodes, or may be merely a central ring.

A solenoid surrounds the envelope and is supplied with direct current for establishing a longitudinal magnetic focusing field in the space between the cathodes. For very low values of this focusing field, the output of the device is substantially proportional thereto, although saturation effects appear at higher values, as will be later explained. The cathode plates are supplied with radio frequency at a frequency of approximately 50 magacycles or higher. The anode is connected through a meter to the positive terminal of a variable potential source.

Applying, say 50 volts of radio frequency on the cathode plates, and gradually increasing the potential applied to the anode from the D.C. source, the meter will indicate currents as shown by the graph of Fig. 8. Up to a certain minimum

FIG. 8.



Anode current against anode voltage characteristic of the electron multiplier tube.

voltage, which depends principally on the distance between the plates, no measurable current will flow. Beyond this point, the current increases with voltage up to a definite point, then decreases again. Further increase of the voltage gives a second curve of the same general form as the first, rising to a maximum and again falling to zero, while still further increase will show a second repetition of the effect, usually of much greater magnitude.

It will be understood that the curves shown are illustrative

merely, no numerical values being shown, since these will vary with the magnitudes of the oscillating potential on the cathodes, the frequency of this potential, the focusing current, and the dimensions of the device. Currents as large as a half ampere have been obtained.

Cutting off the magnetic focusing field stops the flow immediately. Increasing the oscillating potential on the cathodes broadens the range wherein the current flow occurs, eventually causing the curves to merge into a single continuous one having either multiple peaks or mere changes of slope indicating their position. Changing the frequency of the oscillator changes the position of the peaks; i.e., the voltages at which the maxima occur. The same is true as to tubes of different dimensions.

The explanation of these properties is simple in general, although the detailed analysis presents certain obscurities which render its presentation here undesirable. An electron (e.g., a photo-electron) liberated at one of the cathodes is accelerated toward the other plate by the anode voltage, being prevented from striking the anode by the magnetic field which converts the transverse component of its motion into an arcuate one. Its time of flight is determined by the distance between the plates and the velocity imparted to it by the anode potential, plus the integrated effect of the potential between the cathodes during its flight. The latter factor determines whether (1) it will strike the other plate with sufficient velocity to cause emission of secondary electrons (2) strike with a lesser velocity, or (3) fail to strike at all.

Where the first condition obtains the emitted secondaries are accelerated in the opposite direction to generate new secondaries at the cathode plate where the first electron was emitted, and if the ratio of the secondary emission be greater than unity, a multiplication by this ratio will occur at each impact. Under the second and third conditions no multiplication will occur.

It will be noted that with no R. F. in the cathodes, the anode potential contributes only to the mean velocity of the electrons through the tube, and has no effect whatever on the velocity of impact, since the acceleration it imparts to an electron leaving one of the cathodes is exactly neutralized by

the deceleration imparted to the same electron approaching the other cathode.

Although the collection of any individual electron by the anode is improbable, owing to the shape and position of the latter and to the presence of a guiding field, a certain proportion of the total electrons will be collected. This proportion will depend upon the portion of the cathodes which are emitting secondaries (i.e., upon whether the electrons are striking near the center or near the periphery), and upon the transverse component of the electrostatic field within the chamber as determined by the space charge, the curvature of the lines of force between cathodes and anode, and upon any bias which may be applied within the tube.

Eventually, however, a point will be reached where the number of new secondaries emitted is equal to the number collected at each impact, and the current through the meter becomes constant. Within certain limits, therefore, the less the probability of any individual electron being collected, the greater the equilibrium current will be, and hence this current will be increased by increasing the guiding field. A limit to this, however, is the space charge developed when the number of electrons in the cloud which travels between the plates becomes very dense, causing the saturation effect above mentioned. When the space charge is the limiting factor, the largest equilibrium currents are obtained without using any magnetic field.

The peaks in the curve occur when the average time of flight or travel of the electrons is an odd number of half-cycles of the oscillating potential on the cathode, the three peaks shown in Fig. 4 representing five, three and one half-cycles respectively. With a given tube and source of oscillations and range of anode voltage it may only be possible to show one or two of the peaks. Under other circumstances still other peaks will be shown, e.g., the seven or nine half-cycle peaks. The higher the anode potential, the smaller is the time of flight, and hence the peak of output current corresponding to the highest voltage represents a time of flight of one half-cycle. This peak is usually materially higher than the others under otherwise similar conditions of operation.

Where electrons strike the cathodes without emitting

secondaries (condition 2 above); they will subtract from the anode current; where they meet condition 3 they are ineffective except as they increase the density of the electron cloud or plasma, and hence the probable number of electrons collected per half-cycle.

From the above, the reasons for the performance of the device will be clear. The total output current or equilibrium current varies with the proportionate number of electrons falling under conditions 1, 2 and 3, and this in turn varies with the potentials applied.

The uses of the device may readily be deduced from the characteristic curve, which shows alternate regions of positive and negative resistance. These regions obviously permit the modulation of the output by variation of anode potential. Operation in the negative resistance regions permits use as a generator of oscillations of any frequency, as in the case of any of the well known negative resistance devices. Rectification and frequency doubling both occur in the anode branch of the circuit, and may be put to their usual uses. Self-excited oscillations are developed if the inductance connecting the cathodes is tuned to the frequency for which the time of one-half period is approximately equal to the time of flight of the electrons. With a sensitive tube, the shock of closing the anode circuit is sufficient to start the tube oscillating. Unless these oscillations are limited by the anode supply or focusing field, their amplitude builds up until the tube is immediately destroyed.

Multiplying an Electron Flow.—Current multiplication, which is the particular concern of the present paper, may be obtained with this apparatus in several distinct ways, being dependent upon limiting the average number of impacts resulting from a single initial electron so that the total output current remains materially below the equilibrium value.

The first of these modes of operation comprises interrupting the action periodically at such short intervals that the limiting conditions cannot supervene. Since each interval will include the same number of half-cycles, and hence the same number of multiplying impacts, it is clear that the mean output current within the interval will be proportional to the number of initiating electrons liberated in the interval.

A second mode of operation depends upon a "drift" of the electron cloud toward the anode, so that if the first impact occurs on the axis of the tube, the succeeding impacts will occur successively nearer and nearer the anode, until the electrons are finally collected, the "descendants" of the successive initiating electrons reaching the collection point in their proper order, having accomplished the same number of impacts.

Electron multiplication by this "drift" method may be compared to a thermionic amplifier having many stages, and in which there is a great deal of coupling between adjacent stages, a lesser but appreciable coupling between stages farther apart in the chain, and even some coupling between the final output and input. This amount of back coupling is decreased by increasing the intensity of the magnetic guiding field. As the intensity of the field is increased, more and more amplification is possible without the tube "breaking down," that is, output becoming independent of the input. This is so because the electrons are more accurately guided and the "drift" of the electron cloud toward the anode becomes more definite the greater the magnetic guiding field. With an intense guiding field using the "drift" method, the multiplier may show complete linearity between output and input for small input variations, even after the tube has broken down and is passing considerable current that is independent of input electrons.

With a suitably intense magnetic guiding field, almost any degree of amplification is obtainable. A tube employing pure nickel cathodes has been constructed to operate as a photocell, giving an output of nearly a micro-amp. per lumen. Under these conditions by direct measurement the amplification is greater than 5×10^7 times. Whenever possible, therefore, this method is preferred. The action of the multiplier used in this manner is stable and reliable, even up to the very highest gains obtainable.

When it is not possible to employ the intense guiding field required to properly use the "drift" method of multiplication, a third mode of operation is possible which depends upon adjusting the multiplier just under its "break down" point. In this case, the "drift" of the electrons toward the

anode is made as definite as possible, and the amplification of the multiplier increased slowly, either by varying the magnetic guiding field or the amount of radio frequency on the cathodes until the required multiplication is obtained.

In using the multiplier tube in this manner, the action compares somewhat to that of a gas filled photoelectric cell. The multiplication is increased slowly and is left just under the "break down" point at which the output ceases to be dependent upon the input. The amount of multiplication obtainable in this manner depends somewhat on the use to which the cell is being put, but a gain of several thousand may usually be relied upon.

11. APPLICATION OF THE ELECTRON MULTIPLIER TO THE IMAGE DISSECTOR TUBE.

Construction.—A combination of the electron multiplier and the image dissector tube for increasing the sensitivity of the latter, is carried out as shown in Fig. 9. The photoelectric

FIG. 9.

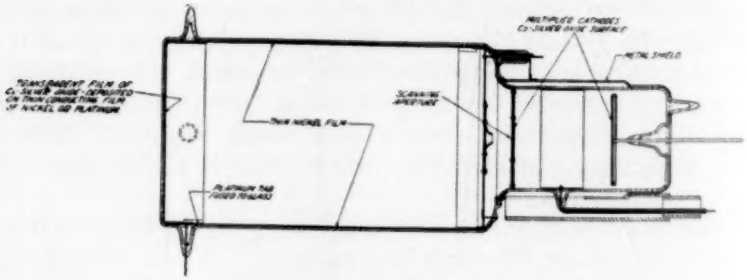


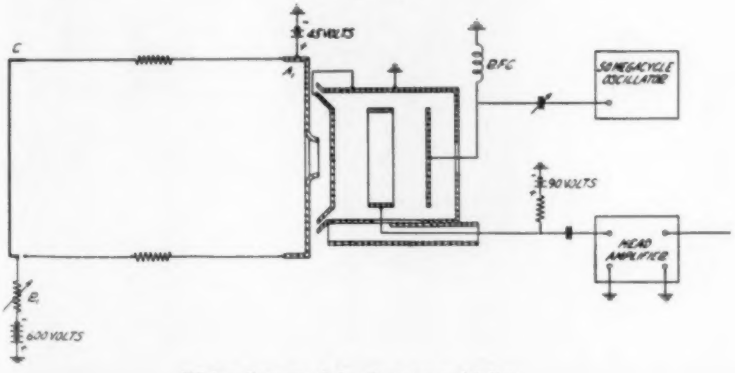
Image dissector tube having a built-in electron multiplier.

cathode onto which the optical image is focused, is formed as a thin translucent film of silver oxide on one end of the glass tube itself. The multiplier is positioned in the opposite end of the tube. The main anode of the dissector is a silver disk having the edge spun over so that it fits snugly into the glass envelope. The anode and cathode are connected together with a thin film of evaporated metal on the glass walls to insure a uniform potential gradient down the tube and to remove wall charges. In the center of the anode disk there is fastened a smaller disk with a protuberant center in which

there is a rather large aperture. The anode protuberance faces the multiplier end of the tube. Behind this main anode is a tightly fitting silver cup, in the center of which the scanning aperture is punctured. This silver cup forms one of the multiplier cathodes. The second multiplier cathode is mounted from a stem and is spaced about six centimeters from the silver cup. Radio frequency is applied only to this second cathode. The output of the multiplier is taken from the anode ring which is supported from the tube walls. A metal shield covers the whole multiplier structure.

After the tube has been mechanically constructed, it is sealed onto the pump and given a good baking until a very good vacuum is obtained; oxygen is admitted and the dissector and multiplier cathodes properly oxidized for formation into photo surfaces. Cæsium pills (not shown in figure) are then flashed in both the multiplier and the dissector chambers to give a proper amount of Cæsium in each of these, after which the tube is given a heat treatment to sensitize these cathodes. The tube may then be sealed off the pump.

FIG. 10.



Wiring diagram of the dissector-multiplier.

Operation of the Dissector-Multiplier.—The multiplier dissector is connected as shown in Fig. 10. A potential difference of approximately 600 volts is maintained between the image cathode and the anode. This potential may be varied by the resistor in series with the cathode for adjusting the magnetic focus of the electron image. The first multiplier

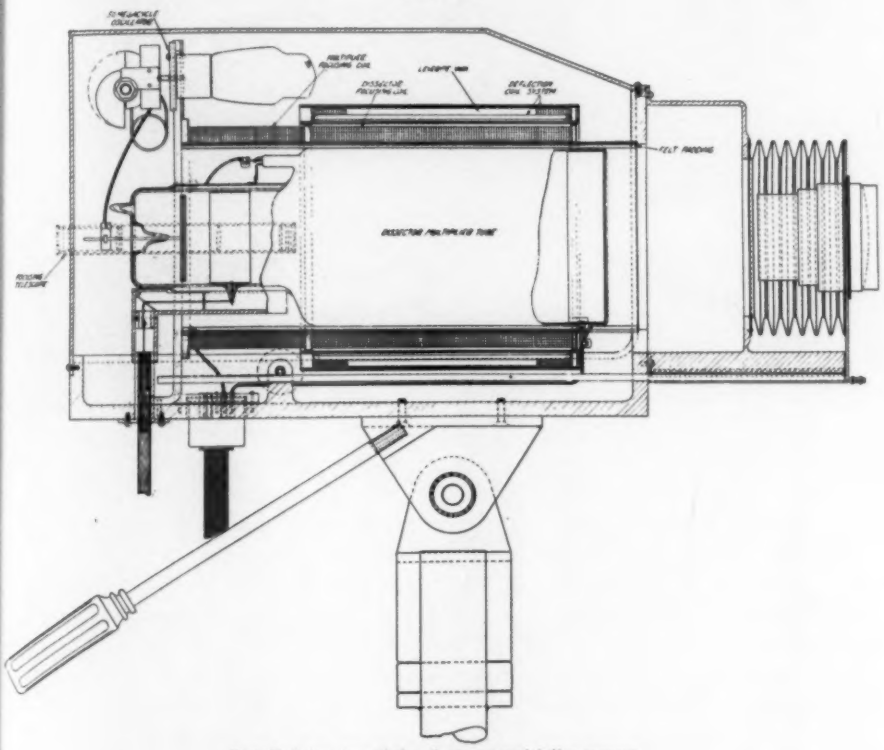
FIG. 11.

Photograph of a dissector-multiplier tube.

cathode is connected to the outside shield and to ground. The main anode of the dissector is maintained about 45 volts positively with respect to the first multiplier cathode and ground, for the purpose of removing the secondary electrons that are knocked out in the immediate neighborhood of the scanning aperture, otherwise these secondaries are drawn into the multiplier and blur the image. R.F. of about 50 mega-

cycles is applied to the second multiplier cathode through a small variable condenser which permits the voltage to be varied from perhaps 20 to 50 volts R.M.S. The anode of the multiplier must be maintained at quite accurately the correct positive potential with respect to the cathodes. This voltage

FIG. 12.

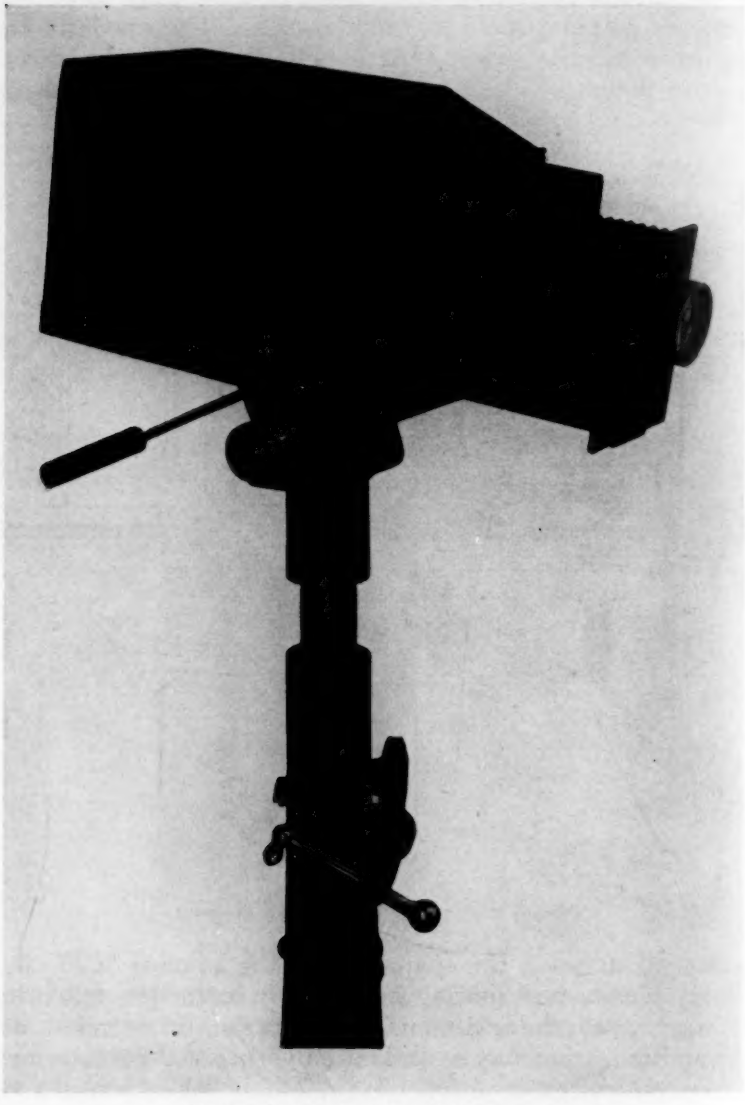


Detailed drawing of the dissector-multiplier camera.

is dependent upon the frequency of the applied R.F., but for six centimeters spacing between the cathodes and using 50 megacycles, the optimum is between 60 and 90 volts, and fair multiplication may be obtained at either of these extremes. The output of the multiplier is preferably taken across a resistor in series with the multiplier anode.

Dissector-Multiplier Camera.—The tube is positioned in a focusing and deflecting coil system as shown in Fig. 12. The

FIG. 13.



Photograph of the dissector-multiplier camera.

focusing solenoid is divided into two parts. One part extends the length of the dissector and the other part covers the multiplier chamber. Both fields have the same polarity so that they are practically equivalent to a single solenoid extending the full length of the tube, except that the current through them may be separately adjusted. Current for these focusing solenoids should be supplied from a good accumulator source through variable resistors that will give close adjustment and firm connection by the variable member. About 500 ampere turns are required in the dissector focusing coil and slightly less in the multiplier coil. The deflecting coils extend the full length of the dissector chamber and may be either inside or outside of the focusing coil. They should not extend quite to the multiplying chamber because this results in the scanning fields modulating the output of the multiplier.

In adjusting the multiplier dissector for the first time, an intense image is focused on the dissector cathode so that a picture may be received without the use of the multiplier. The approximate values of focusing current are noted for the first, second and third magnetic foci, also the effect of the multiplier focusing field on the dissector focusing field. A small amount of radio frequency is then applied to the multiplier cathode, and the multiplier focusing field slowly increased until the multiplier breaks down. The approximate amount of gain is noted just before break-down, and the intensity of the optical image reduced accordingly. This may be expected to be approximately 20 to 100 times. The amount of R.F. applied to the multiplier is then increased slightly and the process repeated. This will result in an increased sensitivity of the tube. The whole process is repeated until the optimum value for the R.F. is determined. After this, the only multiplier adjustment necessary is the focusing. The multiplier focusing also makes a very satisfactory level control.

Sensitivity.—Addition of the multiplier to the dissector in the form shown in Fig. 9 has increased the sensitivity of the latter by three or four thousand times, with no observable increase of the noise level. The sensitivity is now adequate for outdoor scanning even when there is no direct sunlight. It is not necessary to have a particularly sensitive image cathode. Good images have been transmitted direct from

the subject with a tube having a cathode sensitivity considerably less than a micro-amp. per lumen.

In the multiplier used in the present combination, the "drift" of the electrons toward the collection ring is not very definite because of the necessary limitation of the intensity of the multiplier focusing field. The resulting feed-back of electrons from portions of the cathodes where the probability of collection is highest to portions near the center where the probability is a minimum, imposes a definite limit on the amount of multiplication obtainable for stable operation. The sensitivity may be improved to almost any degree by increasing the definiteness of "drift" or by using a two or three stage multiplier. It does not seem too optimistic to expect that eventually *the multiplier will eliminate the entire amplification system up to the modulator of the radio transmitter.*

11. OTHER APPLICATIONS OF THE ELECTRON MULTIPLICATION.

The multiplication principle appears applicable to many uses besides amplifying an electron flow, notably:

1. Image amplifier and translation device for such uses as an infra-red camera for fog penetration, electron microscope or telescope, and general intensification of optical images.
2. Generator of oscillations of any frequency, particularly adapted for frequencies above 20 megacycles.
3. Radio frequency amplification for transmitter use. The output may be readily modulated.
4. Short wave radio receiver.

12. GENERATION AND SYNCHRONIZATION OF THE DEFLECTING CURRENTS.

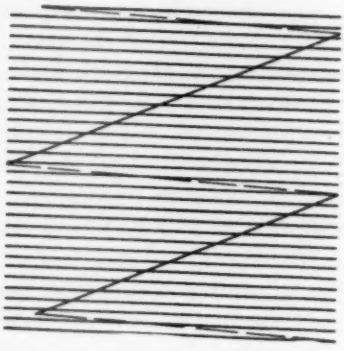
While it would be outside the scope of the present paper to give a detailed report of the method of generation and synchronization of the saw-tooth wave currents used for scanning at transmitter and receiver, nevertheless, a brief description will be included for the sake of completeness.

Modes of Scansion.—Many modes of scanning have been tried and used during the course of the present work, but two modes are especially important and will be considered here.

The first of these methods is shown in Fig. 14, and is perhaps the simplest type of linear scansion that may be used.

The picture field is scanned in the horizontal direction with a saw-tooth wave of comparatively high frequency— f_2, for example—5,000 to 10,000 repetitions per second, and in the

FIG. 14.

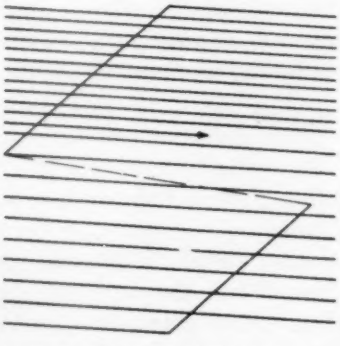


Path of cathode ray beam in simple linear scanning.

vertical direction with a saw-tooth wave of low frequency, f_1, varying between 16 and 30 repetitions per second. The number of scan lines is equal to f_2/f_1.

The second scanning mode is shown in Fig. 15, and may be

FIG. 15.



Path of cathode ray beam in interlaced linear scanning.

called interlaced scanning. This second mode is similar to the first, except that the vertical scanning is at a frequency two or three times faster than would be normally used with simpler

mode and the lines of successive scansions are displaced vertically from each other by one-half the distance between lines.

There are several methods possible for accomplishing the required interlacing of lines. One method [3] is to use a horizontal scanning frequency that is not an exact harmonic of the vertical scanning frequency, but preferably a harmonic of one-half or one-third the vertical frequency. If the horizontal is commensurable with one-half the vertical frequency, a complete picture consists of two scansions, but if commensurability is at one-third the vertical frequency, a complete picture requires three separate scansions.

The principal advantage of interlaced scanning is that flicker may be completely eliminated without too great a sacrifice of picture detail. The disadvantage is that very accurate control of the scanning frequencies is required. Satisfactory means of such frequency control have been developed however, and it appears probable that interlaced scanning will be the type adopted when Television is introduced commercially.

Generation of Saw-tooth Wave Currents.—The deflection of the electron beam at transmitter and receiver is most conveniently obtained electromagnetically. This requires that a saw-tooth wave of current be made to flow through deflecting coils having considerable inductance. In the case of the horizontal deflection where the higher scanning rate is required, the impedance is almost purely inductive.

The equation of a saw-tooth wave is

$$E(t) = k\left(\frac{\sin wt}{1} + \frac{\sin 2wt}{2} + \cdots + \frac{\sin nwt}{n}\right). \qquad (4)$$

Harmonics up to about the tenth are important in the horizontal (H.F.) scanning wave, and up to the twentieth in vertical (L.F.) scanning wave. Thus, we have frequencies as high as 70 kc. to consider in the horizontal scanning current.

Many new and interesting tube generators have been developed to generate these saw-tooth wave currents in coils. In most of these generators a saw-tooth wave of voltage is applied in series with a large resistor (usually the R_p of a tube) across a network the frequency-impedance characteristic

[3] See Egerton's U. S. Patent No. 1,605,930.

of which is similar to that of the deflection coils. This results in a saw-tooth wave of current through the network, and the resulting voltage drop across the network is that required to maintain a saw-tooth wave of current through the deflection coils. The voltage across the network is amplified and the amplified voltage applied to the coils. When the coils are substantially pure inductances, the required network is simply a differentiating network and the voltage which must be supplied to the coils is simply a pulse of short duration.

In another type generator, a pulse is applied to an integrating circuit that changes the pulse into the wave form required to give a saw-tooth wave of current through the coils.

Synchronization of the Deflecting Currents at the Receiver.—Synchronization of the deflecting currents at the receiver is by means of pulses transmitted between lines and between vertical scansions. These pulses are transmitted right along with the picture frequencies, though preferably over a separate radio transmitter. They are separated from the picture frequencies at the receiver simply by amplitude selection. These pulses are applied to the receiving tube in the proper polarity to extinguish the spot during the "back time," that is, the return part of the saw-tooth wave cycle.

13. RECEIVING OSCILLIGHT AND COIL SYSTEM.

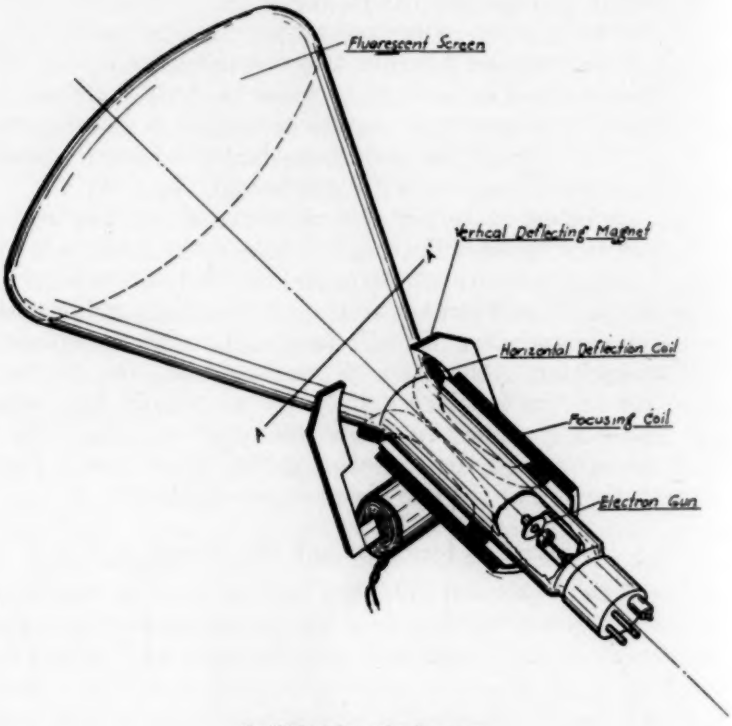
General Description.—Referring to Fig. 16, the receiving system comprises a vacuum tube having an electron gun and a fluorescent screen positioned in a focusing and deflecting coil system. The electron beam from the gun is focused into a small spot on the fluorescent screen magnetically by means of a short coil around the neck of the receiver or "oscillight" tube. This coil extends one-third to one-half the total length of the tube.

The horizontal deflection of the spot for scanning is accomplished by two small coils formed to conform closely to the neck of the tube inside of the focusing coil. Vertical deflection (at L.F.) is preferably accomplished by means of the electromagnet shown. The low frequency coil is wound to suit the impedance requirements imposed by the generator that supplies the low frequency saw-tooth wave current.

Magnetic Focus with a Short Coil.—For the best magnetic

focus the solenoid should extend the full length of the electron's path, but this gives poor deflection sensitivity. It has been found experimentally, that the use of a short focusing coil gives entirely adequate focusing of the spot, and the spot can

FIG. 16.



Oscillight tube and coil system.

be made smaller than necessary. The sensitivity to deflection is so greatly improved by use of the short focusing coil that its use is more than justified. With this kind of focusing, the plane of the electrons' focus is very deep; in fact an approximate focus exists between the plane of A–A^1 (see Fig. 16) and the fluorescent screen, and close adjustment of the current in the focusing coil is not required. Also the beam stays in focus when deflected, which is a great advantage.

Deflecting Means.—It is possible to use several different systems for deflecting the cathode ray beam, when it is fo-

cused magnetically. Perhaps the simplest system is that of an air-core coil positioned as close as possible to the walls of the oscillight tube and preferably inside the focusing coil, as shown in Fig. 16. This type of deflection system is perhaps the most sensitive that can be used, and is to be preferred, especially for use at frequencies of the order of 1,000 to 10,000 cycles per second.

Another type of deflecting system is the electromagnet shown in Fig. 16. The principal advantage in the use of the electromagnet shown, is that the deflection is concentrated at a point along the tube and minimizes the interference, which results into nearby circuits and deflection coils. The ferromagnetic type is preferable for the low frequency deflection. The disadvantages in the iron core type deflection system are that it is less sensitive than the air core, and troublesome distortion results because of the hysteresis of the core material.

When it is attempted to put two deflection systems close together deflecting at right angles to each other, several factors must be taken into consideration. Since the deflections take place simultaneously, it is necessary that all parts of deflection field of one deflecting system be equally sensitive. Otherwise, the deflection produced by this particular system will be modulated by that of the other.

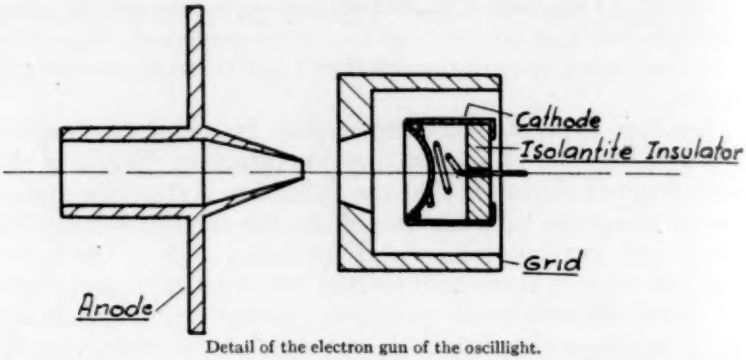
In our present type of deflection system, the high frequency coils deflect the beam first. They, therefore, have to be designed simply to give linear deflection along a line. The low frequency deflection is accomplished by means of the electromagnet placed down the tube a suitable distance from the high frequency coils. These pole pieces must, therefore, produce a uniform deflection, no matter in which part of the tube the beam may be. To accomplish this requirement, the pole pieces have been given the shape shown in Fig. 16, and this suffices if the pole pieces are placed the correct distance apart.

If Keystone distortion is present, caused, for example, by the necessity of mounting the fluorescent screen oblique to the axis of the tube, such distortion is readily and completely corrected simply by suitable shaping the pole pieces of the electromagnet.

It is entirely practical to deflect the beam electrostatically, even though the focusing is obtained magnetically. The con-

dition for satisfactory electrostatic deflection is that the deflection plates must extend practically the full length of the

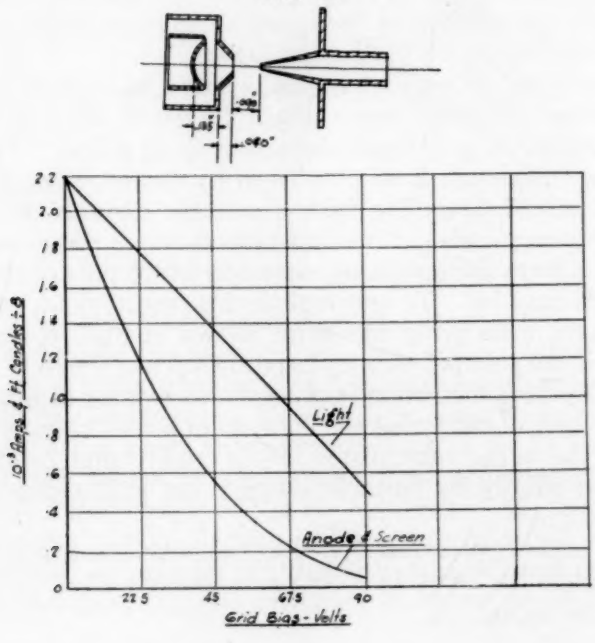
FIG. 17.



Detail of the electron gun of the oscillight.

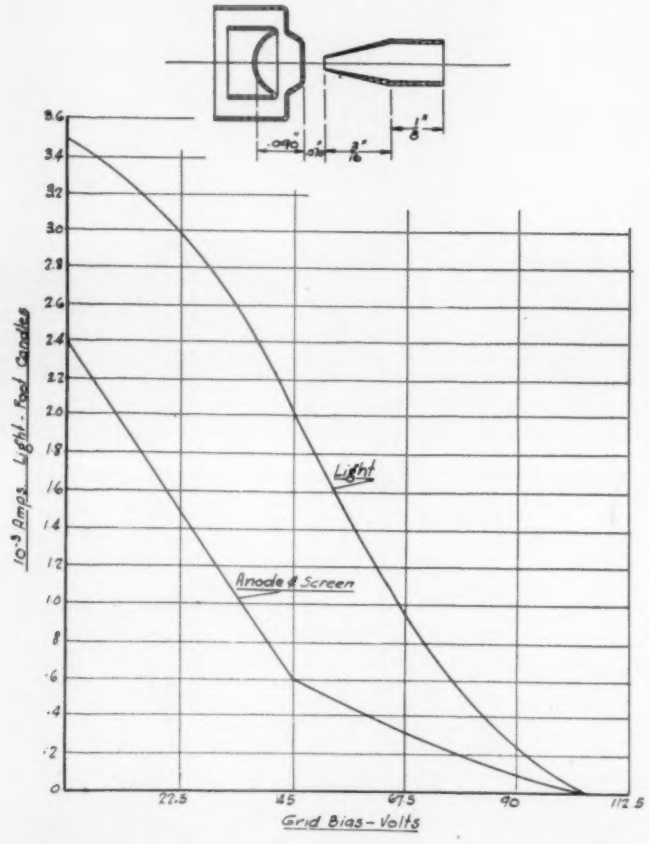
focusing coil. A tube has been built along this line in which the plates are on the walls of the bulb itself. The sensitivity of this particular tube is approximately 100 volts per inch for an anode voltage of 4,200 volts.

FIG. 18.



Electron Gun Design.—The electron gun used in the oscillight is shown in Fig. 17, and consists of a heater type cathode having a concave surface that faces the fluorescent screen. A grid is placed over this cathode, and usually con-

FIG. 19.

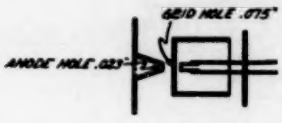


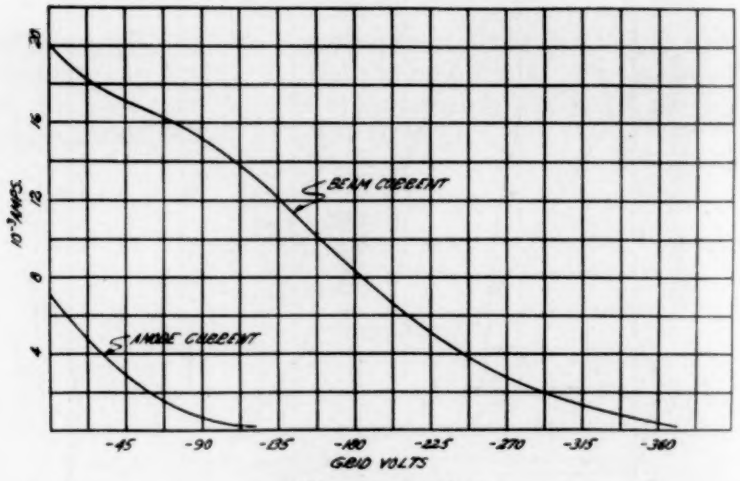
sists of a small cylinder closed at one end and having an aperture in the closed end, through which the electrostatic field of the anode may pass to the cathode. The anode in most of our work has been of the truncated type shown in Fig. 17. The purpose of the protuberant end on the anode is to simulate a point of charge from which the lines of force

originate. This makes it unnecessary to carefully align the anode and cathode.

An idea as to the characteristics obtainable from electron guns built along the general line of that shown in Fig. 17, may be gained by studying the three structures in Figs. 18, 19 and 20, for which the current characteristic and the light characteristic using Willemite are given. The guns shown in Figs.

FIG. 20.





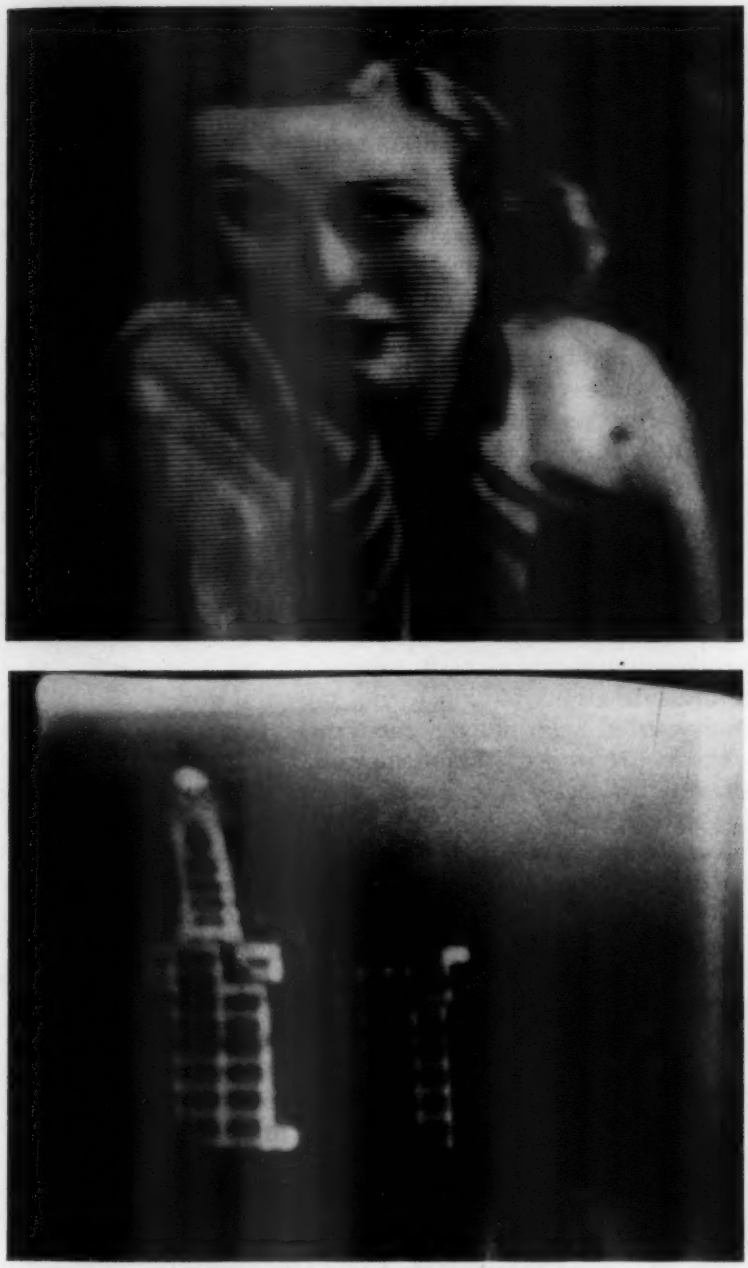
FIGS. 18, 19 and 20.

Curves showing the intensity of the fluorescent spot and the anode plus screen current as a function of the control grid voltage for various electron gun structures.

18 and 19 were designed for use in a large oscillight having a nine inch end. These guns supply as much power at 4,200 volts as can be used with Willemite.

The electron gun shown in Fig. 20, was designed for projection type oscillights, in which the luminescent screen is not a fluorescent screen. It should be noted that over the useful part of the characteristic curve, practically no electrons

FIG. 21.



Photographs of received television images.

strike the anode at all, in spite of the fact that the aperture in the anode is only .020 inch in diameter, and that the maximum beam current is more than 15 milli-amps. With a suitable magnetic focusing system the spot size obtained from this gun is less than .010 inch in diameter.

Miscellaneous Design Factors.—There are many minor factors to be considered in designing an oscillight tube, but perhaps the most important of these is the removal of the charge from the end of the tube. Most of the systems we have used so far depend upon emission of secondary electrons from the fluorescent screen. This requires that the fluorescent material have a secondary emission ratio that is greater than unity for the velocity of the primary electrons used. In addition to this, some means must be provided close to the screen for drawing these secondary electrons away from the screen. Our method of accomplishing this is to evaporate on the walls of the tube a thin film of nickel and to connect this metal coating with the anode.

Another important factor in design of oscillights is to limit the angle of divergence of the beam to approximately 15 degrees, or less. This is important if good magnetic focusing is to be obtained.

Considerations having to do with the design of high intensity tubes using fluorescent screens must take cognizance of the fact that nearly all of the energy in the electron beam is converted directly into heat, and means must be provided for getting rid of this heat. Most of the important fluorescent materials are spoiled completely by over-heating them, and those which are not spoiled instantly show a gradual recrystallization under heat, which decreases their efficiency.

In conclusion I want to acknowledge my indebtedness to the Staff of Television Laboratories for many suggestions and continual coöperation during the course of this development, particularly Mr. Donald K. Lippincott and Mr. R. E. Rutherford, who have contributed greatly to the present work, also Mr. B. C. Gardner who has constructed the tubes, and to Television Laboratories, Ltd. for permission to publish this material.

THE MAGNETO-OPTIC METHOD OF CHEMICAL ANALYSIS.*

BY

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The "Magneto-optic Method of Chemical Analysis," discovered by Dr. Fred Allison [15] of the Alabama Polytechnic Institute at Auburn, Alabama, has been the subject of considerable controversy during the past several years. The great importance and almost unlimited possibilities of the method have warranted this discussion on the one hand while the failure of some experimenters to observe the effects and the lack of any feasible explanation of the phenomena account for the controversial nature of the discussions. The method depends upon the observance or non observance of a slight decrease in the intensity ("minimum") of a beam of polarized light when acted on by a high frequency electro-magnetic field controlled by a motion of a sliding trolley.

In February 1931 the author set up an apparatus roughly similar to that used by Dr. Allison and preliminary observations were begun in order to discover if possible the physical principles underlying the operation of the apparatus. Several not very serious attempts to locate the sharp minima of light attributed by Beams and Allison [1, 2] to a time lag in the Faraday Effect, failed. However minima of a broad type were observed and it was then deemed essential to work with a simpler set-up in order to determine if possible how the plane of polarization of the light is affected by the oscillatory discharge. The results of observations extending until September 1932 are given in a paper entitled "Magneto-optic Rotations by Condenser Discharge," [30] and more fully in a thesis presented in partial fulfillment of the requirements for the Degree of Master of Science at Vanderbilt University by W. M. Breazeale.

* Numbered references are to a bibliography at the end of the article.

During this interval two visits were made to Dr. Allison's laboratory. In spite of the utmost patience and complete cooperation on the part of Dr. Allison and members of his staff, no definite minima dependent on trolley motion and not attributable to spark flicker were observed by the writer. However such work was observed as to form a definitely favorable and optimistic impression in regard to the method and its results.

In September 1932 further experiments were undertaken with two primary objects in view: (1) to check the results previously reported [30] in regard to the proportionality between observed rotations and peak currents, using in this case an Edgerton * mercury discharge tube, controlled by a thyratron, as a source of light and of electrical discharge; (2) to determine definitely the cause of the broad type minima previously observed, and their dependence upon the length and material of the trolley. The results of this work through May 1933 are given in a thesis submitted to Vanderbilt University by Mr. W. B. Brown. Since they are not published elsewhere, these results are presented here in some detail.

In regard to the results of the measurement of the rotation of the plane of polarization of light caused by an electric surge through a single coil, it is sufficient to say that the rotations measured by the half shade polarimeter were in accord with the results expected from the previous work. That is rotations proportional to peak currents were found. In this case the Edgerton discharge tube was used. Since this allowed no reverse oscillation the measured rotations were of the order of several degrees and no difficulty was experienced in the measurement and the results were definite. This allows the conclusion to be reached that in all cases up to the frequencies used of approximately 10^6 cycles, the rotations of the plane of polarization of the light in the Faraday Effect follow the oscillations or fluctuations of the electric current producing the magnetic field, and that the observed rotations are determined by the peak currents which cause maximum brightness of the polarimeter field.

* H. E. Edgerton and K. J. Germeshausen, *R. S. I.*, 3, 535 (1932). Thanks are due to Prof. H. E. Edgerton for supplying tubes and information in regard to their use.

As a result of the second series of experiments it was found that the broad minima are due to the normal Faraday Effect and the minima are reached when the current-Verdet constant product of one coil and cell is equal to this product for the second coil and cell tending to rotate the plane of polarization in the opposite direction, with the ordinary Allison connections. Measurements were made with the polarimeter half shade angle at zero in which case minima of light were observed or more clearly with a small half shade angle when equality of brightness of the fields was attained at the position of equal rotation by the two materials. Three trolleys of No. 16 and No. 30 B. & S. gauge copper and No. 25 chromel wire were used at different times. On the low resistance trolley it was found necessary to use large path differences (of the order of 10 to 20 meters) for the two parallel circuits in order to obtain equal rotations but for the chromel trolley this is reduced to the order of one meter and the minima are sharper and more definite. In all cases the trolley lengths necessary to produce minima were found to be proportional to the Verdet Constant of the liquid in the cell (current through the coil inversely proportional to Verdet Constant). Also for mixtures of two organic liquids single settings were found proportionately between the two settings for the individual liquids. In other words the apparatus behaves perfectly normally and in accord with expectations from the accepted theory of the Faraday Effect. The phenomenon was obviously dependent on the Verdet Constant of the liquids or mixtures used and on the magnetic field strength applied just as would have been observed had direct currents been used. No evidence of any time lag appears from study of these minima. It is interesting to remark that first observations on CCl_4 were not in accord with the Verdet Constant for this substance given in the Smithsonian Physical Tables. Considerable effort was expended to find the cause of this disagreement. It was finally found that the value for the Verdet Constant of CCl_4 given in the Smithsonian Tables is incorrect and that the correct Verdet Constant as found in the International Critical Tables checked with our data.

It is also interesting to remark that upon calling the attention of Dr. Allison to these broad minima and to the

relation between Verdet Constant and trolley setting (a relation which does not hold for his sharp minima), he stated that he had never observed this type of minimum. Upon searching however he was able to check our results with considerable precision and also found the sharp minima at the accustomed settings of the trolleys, in addition to the broad minima. In our laboratory no definite minima of the sharp type, independent of the Verdet Constant but dependent on time lag or atomic weight were observed, although except for the difference in the optical system due to the use of the polarimeter they should have been observed if present. In the daily notes there are occasional remarks in regard to a sharp distinct type of minimum which however could not be located definitely and which was not reproducible. These are now thought to have been due to physiological effects of a type to be described later.

In two years work with the apparatus no experimental evidence had been obtained of any phenomena not readily explained on the basis of the ordinary Faraday Effect and normal operation of the electrical circuits. No evidence of any time lag or factor had been observed. It was therefore thought necessary to attempt to repeat exactly some of Dr. Allison's experiments and for this purpose to construct a set-up identical with his. Mr. J. H. Christensen, who had previously worked several years on this problem at Auburn, was employed and spent four days revising and calibrating the set-up. At the end of this time he expressed satisfaction in the operation of the apparatus and in the presence of the sharp "minima" depending on trolley settings and on the solution in the back cell with standard CS_2 in the front cell. These minima were located by looking in the region where, after careful measurement of trolley lengths, they should be expected on the basis of published values.[15] A number of observations were made and the readings recorded. These readings were then arranged in groups and the average of each group taken as the position of the minimum. The fixed trolley was then set so as to make these positions correspond with the correct scale readings.

Mr. J. A. Peoples, Jr. (technical assistant employed by a

National Research Council Grant-in-Aid) and the author spent two weeks following this without changing any adjustment, in practicing and attempting to locate minima. While "minima" were observed, their appearance could not be correlated with the trolley motion. They were attributed in part to spark flicker although at times with the spark very steady minima were observed but were not reproducible. This was attributed to the lack of practice and to the fact that neither observer had ever been satisfied he could identify a minimum if seen. For this reason request was made of Dr. Allison to allow a visit to his laboratory. This request was readily granted, and Dr. Allison and members of his staff coöperated and aided in every possible way during the days spent in his laboratory. "Minima" were observed almost immediately and settings made with some success by both Mr. Peoples and the author. Apparently the time spent previously in observing the light and in practice settings was an aid since on two previous occasions the writer had not recognized minima. After two days of apparently satisfactory practice, an unknown sample was given us by a member of the laboratory staff. This sample was prepared by adding to distilled water one or more of five chlorides. The test was to look for the minima characteristic of the isotopes of the five elements and to report which were present and which absent.

This test was run in the darkened room. The observer sat at the eye piece, and the staff member who had prepared the unknown sample set the trolley on the position of a possible minimum as requested by the observer. The observer then moved the trolley slowly back and forth over this point while watching for the occurrence of a minimum. The observer recorded whether or not a minimum was seen and requested to be set on the next position, where the procedure was repeated. The position of the minimum for each isotope was examined several times in this manner. (This is the method employed to determine the presence or absence of an isotope.)

The following table represents the individual observations made by F. G. S. and by J. A. P.

(+ indicates minimum observed. − indicates minimum not observed.)

Position of Trolley for Isotopes.[16]	F. G. S.	Reported by F. G. S.	J. A. P.	Reported by J. A. P.
$SrCl_2$				
24.35	+++		++++	
24.46	+++?	Present	++++	Present
24.59	+++		++++	
$CuCl_2$				
20.48	−−−		++?−+	
20.56	−−−	Absent	+?+−+	Present
20.68	−−−		+?+−+	
LiCl				
16.22	−++	Present	−−−	Absent
16.45	+?++		−−−	
KCl				
22.50	++−−	Absent	−−−−	Absent
22.87	++−−		−−−−	
$CaCl_2$				
18.42	+++	Present	++−+	Present
18.66	+++		++−+	

The report made by F. G. S. was correct and at the time was satisfying. The fact that J. A. P. disagreed absolutely in the cases of $CuCl_2$ and LiCl was disturbing. During the observations by F. G. S. he feels that he was unintentionally influenced in two cases by the assistant who was watching the trolley motion and who knew the contents of the solution. Upon the first observation on $CuCl_2$ some doubt was had in regard to the minimum and the observer asked whether the correct region of the scale was being covered by the trolley. The reply "you are covering it very nicely" indicated that no minimum was to be expected, and none was observed. Upon the second run over the lithium isotopes, where the first data were conflicting, the same question was asked by the observer and answered that the trolley motion was rather jerky or too fast so that care was taken and the minimum observed. It is believed that these remarks definitely led to the correct report and that the test was not objective. The report of J. A. P. possibly influenced to some extent in the same manner was only 60 per cent. correct, and the net result of all observations shows 80 per cent. correct, and this result was influenced

at least in the cases of LiCl and $Cucl_2$ mentioned above. It is possible that these observations were still made by inexperienced observers. On several occasions during the previous practice F. G. S. observed minima at points on the scale where none was expected. In each case these were verified by a member of the laboratory staff and attributed to some possible impurity. This is evidence that F. G. S. could identify minima.

Upon return to Nashville a couple of days were required to obtain a satisfactory spark and light and to duplicate the conditions as observed in Auburn. Then minima became clearly visible and in fact appeared more distinct than those observed at Auburn. Prof. H. W. Farwell of Columbia University upon a visit to the laboratory after a stay in Auburn remarked "your minima are certainly no worse than theirs." These minima were found at the expected positions for HCl, NaCl, etc. in H_2O. It was soon found however that minima of the same type occurred at unexpected points on the scale. This was thought to be due to impurities in the liquids and purification of both the water and the CS_2 was attempted by repeated distillation in pyrex. This did not however have the desired effect of removing the minima and suspicion was aroused as to the objectivity of the minima. In order to check this point, first one coil was disconnected and a dummy substituted in its place in the electric circuit, then dummy coils were substituted for both coils, after which the cells and liquids were completely removed and finally the spark itself replaced by a steady light. Following each of these changes minima were observed as previously. Observance of the minima in this case appeared to be aided by the sound of the spark and by manipulation of the trolley control wheel. These minima, obviously subjective, appeared the same as those observed at Auburn. During the above very briefly described experiments many data were taken and many tests made to determine whether an observer knew from the appearance of the minima whether the apparatus was properly connected or not. Under conditions where no change in light intensity was introduced by the change from proper connections to conditions in which no connection was made to the coil the observer could tell no difference in

the minima. Several observers other than the writer and assistant were called in and asked to set on minima. For these observations thanks are due to Dr. P. Rudnick of the Physics Department, to Dr. A. W. Ingersoll of the Department of Chemistry, to Dr. Lyle Lanier of the Department of Psychology, to Dr. Sam Clark of the Department of Anatomy, and to Mr. T. C. Butler, a senior medical student. All of these were familiar with the general problem. Drs. Rudnick and Ingersoll set on minima equally well with and without proper connections to the apparatus when they did not know of changes made. The "subjective minima" mentioned above were described to Dr. Lanier, who identified them as a physiological effect. Dr. Clark and Mr. Butler were told the apparatus was functioning properly and that they should expect minima at certain trolley settings. The former immediately announced that he was bothered by physiological effects such as the unconscious focusing of the dim spot on the fovea, resulting in a dimming. He was told to look for physical effects ignoring this effect and failed to observe any further effect. Mr. Butler set on minima at expected points quite readily and continued to do so when the coil was disconnected unknown to him. Especial thanks are due to Drs. Lanier and Clark for their explanation of possible physiological and psychological effects which might cause the subjective minima. Further observations were made to determine whether this was a periodic effect and it was found that if the observer upon seeing a minimum shifted the eye ever so slightly the effect disappeared but recurred after about three seconds, the period depending somewhat on the color of the light, on the intensity of the light and the observer. At this stage a communication was sent to Dr. Allison describing the above experiments in some detail. He immediately replied very completely stating he had observed these "fatigue minima" and that he avoided them in various ways, by resting the eye, batting the eye, and by rolling the head slightly against the head rest. He is convinced that the minima used in the actual measurements are a real physical phenomenon and are not due to the above causes.

Upon receiving the reply from Dr. Allison the following experiment was tried. In addition to the regular set-up

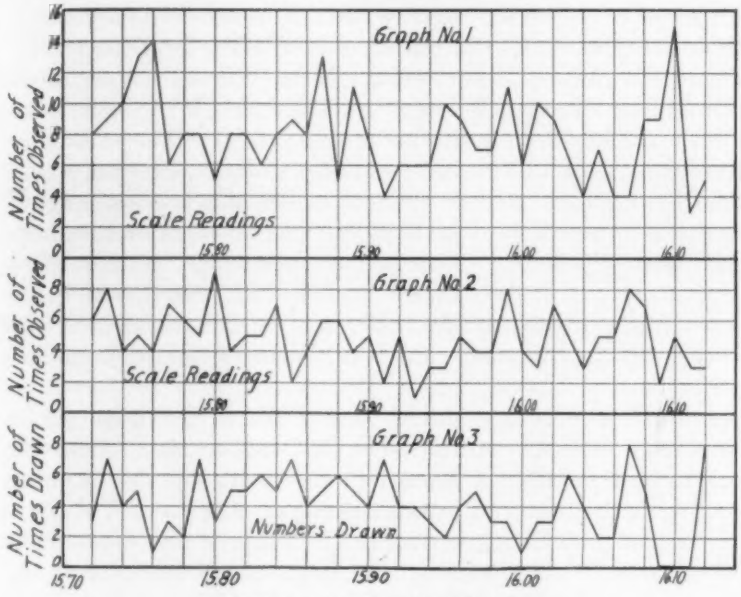
properly regulated, a steady light (automobile head light operated on 6 volt storage battery) was placed so that light from it was reflected by a small piece of mirror through the analysing Nicol parallel to and immediately beside the ray of light under observation. The mirror was placed between the back coil and the analyser and with its plane at 45° to the line of sight. Proper filters and diaphragms were used and the intensity of this spot adjusted to the intensity of the normal spot. After some trials this was accomplished so that the two spots appeared almost identical, and it was agreed that it would be a guess as to which was which. (This in spite of the spark as one source since with a narrow gap it could be made very steady.) Unfortunately it was not possible to place these two fields so that they appeared as a single field and thus it was difficult to fixate both at once. However observations were made and minima observed. If both spots were observed, they would appear to dim together and both return to normal intensity upon a shift in the eye, or more frequently only one spot would be observed when it would dim and be observed as a minimum. This occurred just as readily for light from the steady source not affected by any rotation as for the light observed in the normal manner. A careful search revealed no effect or dimming for one ray which was not similarly observed when viewing the other. The necessary conclusion is that all the observed phenomena were subjective.

During the course of the above work many observations of minima had been made, particularly in efforts to establish the zero point on our scale. For this purpose a dilute solution of HCl was used in the back cell against CS_2 in the front cell (toward light source). With this combination a group of three minima are to be expected at scale readings [15] of 15.75, 15.85, and 16.10, the first two due to the isotopes of Hydrogen in the HCl and the last is an unknown minimum always found on the Allison apparatus and frequently used for calibration purposes. In attempts to locate accurately these minima this part of the scale was passed over many times and the positions of the observed minima recorded. The results of all data taken are shown in Fig. 1, Graph 1, which is plotted in the manner used by Ball and Cooper.[40] The abscissæ

are the scale divisions between which observations were made, and the ordinates represent the number of times a minimum was recorded for each scale reading. This curve shows definite peaks at approximately the expected points for the minima. However many of the data were taken starting at 16.10 as the first minimum and recording this value, then working down the scale with the expectation of finding minima at 15.85 and 15.75 or in the reverse direction starting at 15.75. This is in accord with frequent practice. Experience in turning the wheel to move the trolley enables one to set approximately at desired points. The entire range was not always covered in taking these data. In order to make a definitely objective test after making Graph 1, the following method was resorted to: the trolley was set at some point, unknown to the observer, between 15.62 and 15.72 or between 16.12 and 16.22 and the observer told he was below or above the range to be observed. He then proceeded to move the trolley across the range and to set on and report all minima observed until stopped at 16.12 or 15.72 by the assistant. The positions of all called minima were recorded by the assistant. No remarks were made, and the observer was not informed of the results of his run during the duration of the entire test. After one run over the scale the observer and assistant exchanged positions, and the procedure was repeated until a total of about 250 minima had been recorded in two days run. These data were taken in good faith by both observers, working under best conditions of rest and freedom from strain. The results plotted in the same manner as those in Graph 1 are shown in Graph 2, Fig. 1. No such pronounced peaks occur as in Graph 1. The distribution is about what should be expected on the basis of a chance distribution such as is shown in Graph 3, Fig. 1. In this case fifty numbers from 15.67 to 16.17 were placed in a box and withdrawn in groups of five which after being recorded were replaced in the box before the next drawing. While this does not exactly simulate the method of reading minima, it is close enough and indicates the possibility of at least such peaks as occur in Graph 2, being due to chance when the total number of observations is approximately 250. It is interesting to note that during the above described objective test that on several occasions one

of the terminals to the back coil was disconnected unknown to one observer (J. A. P.). In spite of the slight change in light intensity due to this (compensated for when possible without arousing suspicion, by reduction of spark or control Nicol) the observer did not know until told the following day that this had been done. However the average number of minima observed per run over the range in this case was 2

FIG. 1.



Distribution of scale readings of minima.

as compared to 2.7 with the normal condition. While granting the possibility of this being due to objective minima observed, it is believed to be due to the change in light intensity in this case or to chance since sufficient tries were not made to eliminate this factor.

Upon a visit to our laboratory Dr. C. R. Fountain of George Peabody College called attention to the literature on the N-ray phenomena of Blondlot.* It is of interest that this specific case of subjective phenomena was not known to us pre-

* R. Blondlot, Paris, *C. R.*, 136, 1227 (1903).

viously. In three articles in the JOURNAL OF THE FRANKLIN INSTITUTE for 1907, G. F. Stradling * gives a very excellent resume of the N-ray literature and in the third of these papers outlines the probable effects leading to these observations. It is our opinion that all the above described tests and results in observing the sharp minima may be explained as due to the physiological and psychological causes which have been accepted in explanation of the N-ray phenomena. It would require a summary of the 50 papers listed in the bibliography at the end of this article and of the 278 papers listed by Stradling to enumerate all the points of similarity between the observance of these minima and the N-ray effects. Some of the outstanding ones may be mentioned: small field of vision; faint light; work in dark; muscular activity to produce effect; observance of both minima and maxima of light; effect observed at fairly regular intervals along a scale; effect is a change in intensity (a kinetic not a static effect); effect of atmospheric conditions (humidity, barometric pressure, temperature); necessity that the observer be experienced, rested, and in the proper frame of mind; effect of X-rays; accumulative effects (storing of N-radiations and isotopic influence on minima); etc.

A further visit was made to Auburn in the hope that a completely objective test in the laboratory there might be made. Our opinion in regard to our work on this problem along with the material presented above was frankly given to Dr. Allison and his co-workers. In view of the friendliness and coöperative attitude at all times of all workers at the laboratory and of the efforts of Dr. Allison himself to aid in any requests whatever this was not an easy task. (Our files contain over thirty pages of personal letters written by Dr. Allison in response to inquiry at various times.) The criticisms were received graciously. The requests for objective tests in that laboratory were refused as a matter of policy. The basis for this policy was that sufficient objective observations had been made and that it is now up to other laboratories to produce contrary evidence. It did not seem reasonable to refuse to perform one simple objective test suggested, namely: the observer was simply to tell whether or not the entire trolley

* G. F. Stradling, JOUR. FRANK. INST., 164, 57–74, 113–130, 177–199 (1907).

system was disconnected, the connection to be made or broken behind a screen in such manner that no suggestion could be given the observer. (Attention is called to the N-ray observer who unconsciously detected whether the shutter was "offen" or "geschlossen" by the sound of the operator's pencil in writing "o" or "g.") This test should be made on a set-up in use at Auburn on which removal of the trolley would not affect the light intensity.[49] In this modified set-up the first order lower frequency surges do not pass through the trolley system, which is coupled to the discharge circuit capacitively. This test as a preliminary to determine if further specific objective tests might be advisable was refused on the ground that none of the observers would be competent under the mental strain involved in a test of so great import. Therefore it seems necessary to report the results of the tests made in this laboratory as we have attempted to do briefly and without too much detail of the actual work done or observations made.

It may be worth while at this point to mention several inconsistencies or incongruities in the material at hand. Allison states[3, 15] that differential wire paths are found dependent on the materials in the two cells, and that substances mixed together exhibit their individual minima even when present in concentrations as low as one part in 10.[11] In this case one would expect to find for a mixture of two liquids in the front cell two minima characteristic of each impurity in the back cell. Since sodium and magnesium occur so promptly as impurities[52] in the back cell it is reasonable to suppose that some impurities would also exist in the front cell. For each of these impurities in the front cell one should expect a minimum for each isotope in the back cell. If this is the case the minima have not been so interpreted. In the modification of the apparatus mentioned above rotation of the plane of polarization of the light by the CS_2 in the front cell is not required to produce the light. Thus CS_2 should have no preference over impurities in producing minima against the material in the back cell (concentration has been shown to have little effect[37]).

The modified form of the apparatus[49] in which the trolley and coils are connected to the main discharge system only

through small condensers (.0001 to .002 mfd., though the actual magnitude of this capacity seems to be of little or no importance) is so great a departure from the original set-up electrically as to rule out immediately the Faraday Effect as a possible cause of the phenomena. The high frequency which passes the filter system has no noticeable effect on the light passing the Nicols unless to produce minima of the type described by Allison. Certainly the energy arriving at the coils is materially decreased. In this case the light is obtained by uncrossing the Nicols and then using a third Nicol to reduce it to working intensity. The back coil is reversed from the ordinary connection to produce minima with the uncrossed Nicols. Real minima in this case could be due only to a resonance effect between a natural frequency of the circuit and the molecular radicals themselves. It seems improbable that sufficient energy should be absorbed by the ions to show characteristic effects throughout so wide a range of concentrations. It also seems unlikely that the wave-length of the light [3] traversing the liquid should affect such a resonance frequency.

In regard to the effect of atmospheric conditions there are conflicting reports. Mr. Christensen stated that the barometer reading seems to be more important than the humidity while other workers ignore the effect of pressure. High humidity seems to affect the apparatus at Auburn and is combated by the use of heaters so that the laboratory is frequently very hot.[15] Other workers state that they are not bothered so much by humidity but find it necessary to cool the liquids in order to work satisfactorily. Work in this laboratory has not been done with extremes of humidity, but no effect has been seen between the ranges of 40 per cent. and 75 per cent.

During the writing of this report the paper by Webb and Morey [45] "On the Existence of Time Lags in the Faraday Effect" has come to our attention. The results reported by these authors are in accord with the observations outlined above and published in more detail in the theses mentioned. Webb and Morey however do not record the finding of physiological effects unless their statement "other minima reported by the observers were due to random and spurious influences" refers to such.

The existence of real minima, which we as yet have been unable to see, is of course indicated by the large mass of data published from various laboratories. It is hoped that this paper shall serve as a warning to the uninitiated. It is also hoped that experimenters in this field will realize the necessity for further objective study and that they will publish the results of such tests in great detail, giving completely the methods of making observations, all individual readings, precision measures, etc. Even though the objectivity of the method be eventually established if this paper serves these two purposes it will have served to good purpose.

In conclusion we are due to express our greatest appreciation to Dr. Fred Allison for his continued interest, information, and coöperation in this work, and this appreciation is expressed with pleasure. It is with real regret that it seems necessary to report the negative results, since this problem was undertaken and carried out in hopes of finding a real physical basis for the Magneto-Optic Method of Chemical Analysis. Experiments in this laboratory however indicate that the phenomena observed here are best described as a type of the N-ray phenomena. The excellent third paper of Stradling * makes it unnecessary for us to go into the possible physiological effects which may account for the apparent consistency of results. It is recommended that any one interested in this or other phenomena dealing with dim lights read this article.

We are indebted to the National Research Council for a Grant-in-Aid which made it possible to continue this work through the summer of 1933, and to employ the very helpful services of Mr. J. A. Peoples, Jr., whose assistance has been of exceptional value. We also wish to express appreciation to Dr. Philip Rudnick for many helpful suggestions.

October 25, 1933.

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* G. F. Stradling, ibid.

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EMISSION OF ELECTRONS FROM COLD METAL SURFACES.

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ABSTRACT.

The results of Del Rosario on cold emission are shown to result from the fact that, through the attainment of specially good vacuum conditions and freedom from contamination, the cold electron current from the wire was suppressed up to field strengths greater than those at which measurable current was obtained in the work of previous observers. It was in fact suppressed beyond the point at which discharge started from points on the welding of the support. The results of Del Rosario are thus probably characteristic of conditions other than those pertaining to the wire, a conclusion which accounts for his current voltage curves. The formula of Fowler and Nordheim is found to apply in the measurable regions in form directly and in magnitude only if irregularities of the surface are such as to raise the field at the surface to a sufficiently high value. Breakdowns after intense heat treatment are studied and found to be caused by immeasurably small currents of electrons knocking ions from the anode. The erratic original curves obtained by previous investigators are found to be caused by successive breakdowns similar to those found after intense heating.

1. INTRODUCTION.

Several investigations on the emission of electrons have been completed and published in the last few years. The papers of an experimental nature most frequently referred to in connection with this subject are those of Rother,[1] Millikin and his collaborators,[2] and General Electric Company, London.[3] Beside these there are the papers of Del Rosario.[4] These, however, are not so frequently referred to because the results emphasized in these papers disagree with the later theories and with the results of other investigators. Another

[1] Rother, *Ann. d. Physik*, **81**, 317 (1926).

[2] Millikin & Eyring, *Phys. Rev.* **27**, 51 (1926); Eyring, MacKeown & Millikin, *Phys. Rev.*, **31**, 900 (1928); Millikin & Lauritsen, *Proc. Nat. Acad. of Sci.*, **13**, 45 (1928).

[3] Research Staff of the Gen. Elec. Co. Lond., *Phil. Mag.* (7), **1**, 609 (1926).

[4] Del Rosario, JOUR. FRANK. INST., **203**, 243 (1927); ibid., **205**, 103 (1928).

investigation, that of Bennett,[5] should be mentioned, although its primary purpose was the application of the phenomenon to the insulation in the construction of high voltage x-ray tubes.

Most of these investigations were conducted using tungsten as the metal surface from which the electrons were drawn. Bennett however also used copper and other metals.

The early theoretical investigations were limited to the equation of the Schottky effect for thermionic emission,[6] namely

$$I = AT^2 e^{-\epsilon/kT[\varphi-(F\epsilon)^{1/2}]}, \qquad (1)$$

where I is the current per unit area, A is a constant, ϵ is the electronic charge, k is Boltzmann's constant, T is the temperature, φ is the thermionic work function, and F is the potential gradient at the surface of the emitting electrode. On substituting the values of the constants in Eq. (1), it is found that at room temperature, namely 300°, the field necessary to produce measurable field currents is of the order of 10^8 volts per cm. Eq. (1) predicts a linear relation between the logarithm of the current and the square root of the field. Further it predicts a distinct variation in the emission with temperature.

In all of the experimental investigations, field currents were obtained at calculated fields of the order of 10^5 to 10^6 volts per cm. When the logarithms of these field currents were plotted against the calculated fields the resulting curves were distinctly non-linear.

Also, the experimental investigations of several of the investigators mentioned above and more recently those of Ahearn [7] have shown very definitely that the variation of the current with temperature in the neighborhood of 300° C., if indeed it has any measurable value at all, is not nearly as great as that predicted by Eq. (1). The aggregate of these failures of Eq. (1) left the theory supporting it in a very unsatisfactory condition.

No other theory was developed until after Sommerfeld's

[5] Bennett, *Phys. Rev.*, **37**, 582 (1931).

[6] Schottky & Rothe, Handbuch d. Expl. Physik, **13**, pt. 2, p. 261.

[7] Ahearn, *Phys. Rev.*, **43**, 1058 (1933).

and Pauli's application of the Fermi-Dirac statistics to the electron gas in metals, an application embodying the recognition of a potential barrier at the surface of the metal. Using this theory as a basis for the application of wave mechanical principles, Fowler and Nordheim [8] calculated the emission to be

$$I = \frac{\epsilon}{2\pi h}\frac{\mu^{1/2}}{(\varphi + \mu)\varphi^{1/2}} F^2 e^{-4/3k\,\varphi^{3/2}F}, \qquad (2)$$

where

$$k^2 = \frac{8\pi^2 m}{h^2},$$

h is Planck's constant, m is the mass of an electron, and μ is the energy of the electron gas inside the emitting metal at zero temperature. μ is the usual parameter defining the gas in the Fermi-Dirac statistics.

Formula (2) predicts no variation of I with temperature, and so is in harmony with experiment in this matter. This fact results from the lack of sensitivity of Fermi-Dirac statistics to temperature at ordinary temperatures, a fact which justified the complete neglect of temperature variation in the theoretical development of (2).

The foregoing formula predicts a linear relation between the logarithm of the current and the reciprocal of the field. Before this theory was developed Millikin and his collaborators in one of their papers (2) had suggested that an empirical formula of this form would explain all of their data. However, equation (2) does not predict a measurable current until the field is of the order of 10^7 volts per cm. This value of the field is more nearly in agreement with the experimental values of from 10^5 to 10^6 volts per cm. than the field predicted in the older theory. Nevertheless the calculated fields were not near enough to the theoretical value to confirm the theory. The theory therefore seems to fail only in the order of magnitude of the intensity of the field necessary to cause emission.

To bring the experiments into agreement with the theory, it was logical to attempt to show that the field calculated from the geometrical dimensions of the emitting surface was

[8] Fowler and Nordheim, *Proc. Roy. Soc.*, **119**, 173 (1928).

not the true field at the surface. Another possibility of explaining this failure was that the effective value of φ in Eq. (2) was changed owing to the existence of a surface film of a substance, the work function of which was lower than that of the base metal. Stern, Gossling, and Fowler [9] have considered these two possibilities. They found, theoretically that the films would produce only a minor effect, a conclusion which agreed with the experimental results of an earlier experiment by Gossling [10] in which a film of sodium was allowed to form on the cathode. Further, they explained the discrepancy between the calculated field and the theoretical field on the basis that the emitting surface was a projection on the cathode. The emission was treated as though it came from an effective area over which the field was constant while it was zero over the remainder of the surface. This represents the first approximation in calculating the current from a projection on a body in an electric field. As long as the field at the apex of this projection is large compared with the field at the surface of the rest of the cathode, this effective area is of the same order of magnitude as the true area of the projection. Stern, Gossling and Fowler chose the value of φ to be compatible with their theory of films after estimating the film thickness. Then assuming that Eq. (2) explained the phenomenon, the correction factor for the field calculated from the geometrical dimension of the electrode and the effective area of the projection were determined from the slope and ordinant of the graph of the log of the current vs. the reciprocal of the field. The results of these calculations made on earlier experimental data gave values of the area which were of reasonable magnitude.

It has been pointed out above that the work of Del Rosario did not agree with these theories. This should be qualified to some extent. The form and voltage necessary for any one curve of his work agreed with the other investigators. In addition to this he found that there was no essential difference in this emission curve plotted against the potential difference when the calculated field was changed. He therefore concluded that the emission depended on the

[9] Stern, Gossling and Fowler, *Proc. Roy. Soc.*, **A 124**, 699 (1929).
[10] Gossling, *Phil. Mag.*, **1**, 604 (1926).

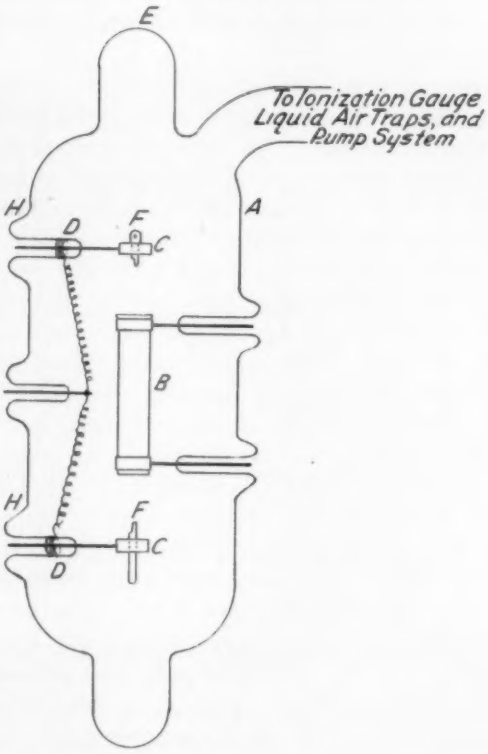
potential difference rather than the potential gradient. Since the other investigators could get no agreement between different wires even of the same size, this work of Del Rosario indicated a failure of the theory.

The motivating purpose of the investigation to be reported in this paper was to clear up these contradictions.

2. REPETITION OF THE EXPERIMENTS OF DEL ROSARIO.

In order to explain these contradictions it was decided to repeat the work of Del Rosario under conditions as nearly as

FIG. 1.



possible the same as those of the original experiments. The tube used by him was obtained, but owing to age and possibly due to rough handling in storage, it leaked very badly.

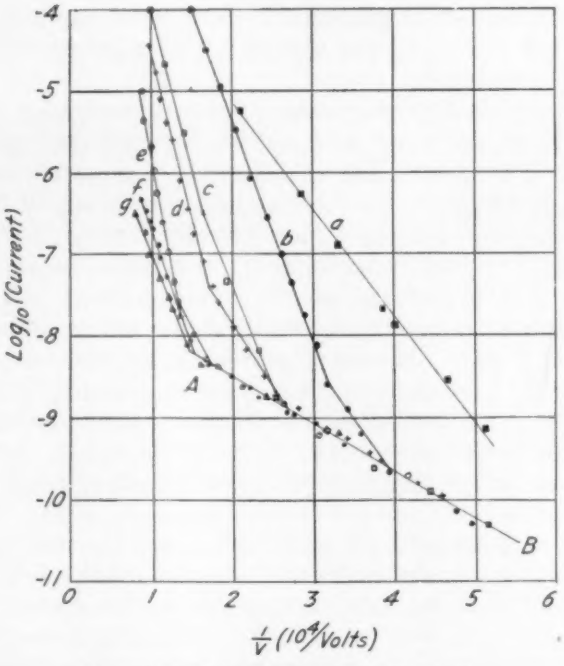
A new tube was therefore made using the metal parts out of the old one. The metal parts were polished since they were badly corroded. These details, though seemingly unimportant have a direct bearing on the results obtained from this tube. The diagram in Fig. 1 shows the construction of this tube. It consisted of the Pyrex glass tube *A* with the nickel cylinder *B* supported by seals. The blocks *C* were drilled to support the lugs *F* to which the wire was welded. The wires *D* were wound around the glass to act as guard rings and prevent the current from leaking across the inside surface of the glass. Likewise the wires *H* were wound around the glass to prevent the current from leaking across the outside of the glass. The end of the tube could be blown out at *E* to remove or replace the wire.

The gas pressure was measured by means of an ionization gauge for the low pressures and by means of a McLeod gauge for the higher pressures. The field currents were measured by means of a galvanometer. The potential at the tube was measured by means of a series of 10 megohm IRC resistors. These resistors were calibrated in sections, the number of resistors in each section being such that the voltage across any section was less than 2000 volts for the highest value of the total voltage applied to the whole series. Sections of this size were calibrated by comparison with a measured current through two one megohm boxes. The calibrations taken in this way were added. When these resistance elements were mounted in paraffin covered glass tubes, the calibration curve had a drop of about five per cent. from the straight line calibration at 10,000 volts. Throughout the whole series of experiments this calibration was checked from time to time. The maximum deviation from the original calibration was less than one per cent. The current taken by this voltmeter for maximum voltage was approximately 40 microamperes. The voltage supply was obtained from a rectified and filtered source. The filter condenser was such that the ripple voltage was less than one per cent. for all of the currents drawn out of it.

The evacuation process consisted in baking the tube and one of the liquid air traps at 500° C. for a minimum of four hours while liquid air was on the other trap. The furnace

was removed and the metal parts were heated to a dull red heat sometimes by means of an induction furnace, and sometimes by electron bombardment from an extra filament (not shown in the diagram of the tube). After this the furnace was replaced and the temperature was again raised to 500° C. for several hours. The liquid air was then moved to the trap which had been heated. This process brought the pressure down to about 10^{-8} mm. of Hg as read on the ionization gauge. Seven wires were used from three spools of tungsten. The

FIG. 2.



Current from tube before supports were shielded.

wire sizes were 0.5, 1.6, and 2.5 mils diameter. The current and voltage were measured for various pressures ranging between 10^{-6} and 10^{-8} mm. of Hg. When the logarithm of the current was plotted against the reciprocal of the calculated field, the data formed a heterogeneous group of straight lines. When the logarithm of the current was plotted against

the reciprocal of the voltage instead of the field, the lines appeared like those in Fig. 2. The two lines to the right marked *a* and *b* in Fig. 2 represent the current in the case of sample experiments using 0.5 mil wire. The two lines *c* and *d* represent the current in the case of sample experiments using 1.6 mil wires. The three lines *e*, *f*, and *g* represent the current in the case of sample experiments using 2.5 mil wire. For small currents these lines converge and coincide to form the line *AB*. The line *AB* represents very closely the results of Del Rosario. The lines above the line *AB* are very much like those of other investigators who used a cylinder and a fine wire to obtain the intense potential gradients. From this graph one would expect that a current was flowing which masked the current from the wire itself.

To test this hypothesis the tube was evacuated with the lugs in the supports but with no wire between the lugs. Current did flow under these conditions. The graph showed a line similar to that of the limiting line, *AB*, in Fig. 2, but with the voltage decreased by a factor of about two.

It would be most remarkable if the leakage across the tube was of such a character that the current-voltage plot of that leakage was such as to give a straight line when plotted in the manner of Fig. 2. It seemed probable that this current was due to cold emission from some point on the supports. This hypothesis explained why the emission was dependent upon the potential difference between the cylinder and the central system and was apparently otherwise independent of the field as calculated from the dimensions of the wire. The fact that currents occured at lower voltages when the wire was absent is to be attributed to the reduction in the field near to the support resulting from the proximity of the wire.

Assuming that Eq. (2) is correct, the area of the emitting surface can be calculated from the curve formed when the logarithm of the current is plotted against the reciprocal of the field. Following Stern, Gossling, and Fowler, the field in Eq. (2) is replaced by the product of the field factor and the potential difference. Assuming that the work function and the Fermi-Dirac parameter of the gas remain the same Eq. (2) becomes

$$I = A\alpha\beta^2 V^2 e^{-b/\beta V}, \qquad (3)$$

where a and b are constants, V is the potential difference, β is the field factor, and A is the area of the emitting surface. For algebraic convenience let us substitute $x = V^{-1}$. The $\log I$ then becomes

$$\log I = \log A + \log a + 2 \log \beta - 2 \log x^{-(b/\beta)x}. \quad (3a)$$

Differentiating Eq. (3a) with respect to x

$$\frac{\partial \log I}{\partial x} = -\frac{2}{x} - \frac{b}{\beta}. \quad (3b)$$

Eliminating β between Eqs. (3) and (3b) and solving for A the area becomes

$$A = \frac{I}{c}\left[x \frac{\partial \log I}{\partial x} + 2 \right]^2 e^{-\left(x\frac{\partial \log I}{\partial x}+2\right)} \quad (4)$$

where $c = ab$ a constant.

Thus, if the area were to remain the same for the projection on the support after the wire was removed as it was before the wire was removed, the product of the reciprocal of the voltage and the slope of the curve would have to remain the same for the same value of current. This is just the relation between the two curves obtained.

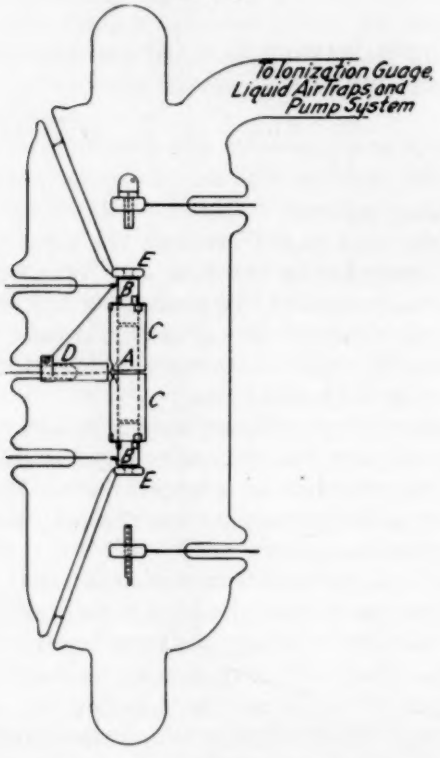
It is obvious that emission from a constant projection is sufficient to account for the experimental results. It is of interest to observe that it is necessary on the basis of the Fowler and Nordheim theory that the emitting area remain constant in the experiment.

Therefore it is justifiable to assume that the limiting line, AB, in Fig. 2 is due to a cold emission from some point on the supports of the wire. When the tube was rebuilt, these supports were polished. This polishing probably changed the projections which were on the support when Del Rosario used the tube. The foregoing supposition suffices to explain the results of Del Rosario. It also explains why his results with different wires fell upon a single curve when the log of the current was plotted against the voltage. The wire, in fact, played no part in the phenomenon in those experiments in which, by thorough baking, its emission had been reduced below that of the supports. The supposition also serves to

correlate Del Rosario's results with those of former observers; although in view of the fact that they used apparatus very similar to his, it is surprising that they did not find corresponding results.

In connection with this matter, the following comment should perhaps be made. Before the experiment of Del Rosario, emission had been obtained from wires at fields of the order of 200,000 volts per cm. as calculated from the dimen-

FIG. 3.



sions of the electrodes. The primary purpose of Del Rosario's original experiment was to see whether this field could be pushed to higher values without the production of measurable currents. He succeeded in pushing the field to the apparent value of 500,000 volts per cm. at which field currents

appeared. It now seems that for the wire that he conditioned, the field at which measurable currents were obtained from the wire itself, was even higher than this. The conditioning of the wire and the corresponding vacuum conditions were such as to result in actual suppression of the current from the wire itself up to above the value at which the actual discharge occurred.

For the purpose of verifying further the following conclusions, and for the purpose of extending the results, it was thought to be advisable to design and build a tube which would eliminate these extraneous currents. The diagram of a tube built for this purpose is shown in Fig. 3. In this tube the lugs to which the fine wire was to be welded were drilled longitudinally so that the wire could be passed through the lug and welded at the end away from the anode and at a place where there was no field. The central cylinder from which the current was to be measured was mounted on a single support at *A*. To decrease the end effects and to collect the electrons moving along the fine wire, the guard ring cylinders were placed as shown at *B*. To prevent currents from reaching the cylinder from the outside and from reaching the support, the cylinders *C* were mounted on the guard ring cylinders *B*, and the cylinder *D* was mounted on the glass seal. The cylinder *D* served also as a guard ring to prevent currents from leaking across the inside surface of the glass. A small leakage was found when the tube was exhausted without a wire. However when glass beads of the form of doughnuts were mounted at the ends of the cylinders *B* as shown at *E*, no leakage could be measured.

Using different wires of the same size and of different sizes, no dependence on the voltage of a kind to resemble the results of Del Rosario could be found. Therefore it is concluded that the dependence of the emission on the voltage as shown by Del Rosario was due to extraneous emission currents.

3. THE EFFECT OF INTENSE HEAT TREATMENT ON THE EMITTING SURFACE

Since the dependence on voltage has been discounted, as shown in the last section, all of the experimental information could be explained on the basis of the theoretical Eq. (1) by means of the simple assumption of small projections on the

emitting surface. As was pointed out in the introduction this leaves two disposable constants so that only the form of the equation is substantiated. In order to have these constants approach a known value, it was necessary to remove the projections from the wire so that the wire became a true geometrical cylinder. Intense heat treatment immediately suggested itself. The recrystallization of the metal would at least form similar projections on the surface of different samples of the same wire.

Various temperatures were applied to the wire by passing a heating current through the wire. No regularities in the results which were not reported by other investigators were noticed until the temperatures were raised to about 2800° K. After the wire had been treated by a heating current which raised the temperature to 2800° K. the potential was slowly raised. No current was measurable until the voltage was such that the measured field was several times as large as that necessary to give emission before the wire had been heat treated. When current first flowed it jumped from a current of less than 10^{-11} amperes to a current of the order of 10^{-4} amperes. This spontaneous type of increase in current will be referred to in the remainder of this paper as a breakdown. Bennett reported breakdowns very similar to these in the case of hand polished electrodes. However the breakdowns reported by Bennett occurred at measured fields very much lower than those observed by the writer. One would expect the surface obtained by intense heating to be smoother than the surface obtained by hand polishing. Not only would the surfaces be smoother but also they would be freer of foreign materials. This breakdown suggests that the surface is changed in some manner since, after the breakdown, a curve was obtained not unlike the curves of other investigators. The breakdown could not be obtained again until the wire was again heat treated.

The problem as far as insulation purposes go, resolves itself into two problems, namely before breakdown and after breakdown. The curves after breakdown taken during this investigation were so nearly identically the same as those of the earlier investigation that the previous investigations can be considered as experiments on the second sub-problem.

The ability to repeat the curves which was discussed by some of these investigators really belongs to the first sub-problem. This will be discussed in a later section. Here the discussion will be limited to breakdowns after heat treatments.

Breakdown after heating was studied in the case of four different wires. Two of the wires were 0.5 mil diameter and two were 1.6 mils diameter. The process of the experiment was to heat the wire in vacuum for 30 min. to 2800° K. The potential at which breakdown occurred was noted, then the process was repeated. Several breakdown voltages were recorded for each wire. The breakdowns were found to occur at about 11,000 volts for the 0.5 mil wires and at about 23,000 volts for the 1.6 mil wires. The breakdowns all occurred within ten per cent. of the average values stated above for different wires as well as for the different readings of the same wire.

The result of a breakdown is certainly the formation of projections at the surface of the wire. In fact Bennett has reported observing microscopically crater-like ruptures on sharp points used as cathodes after a breakdown. It was not known whether the rupture at breakdown is caused by electrostatic forces on the surface of the wire or by the bombardment of the cathode by positive charges. These positive charges might be caused by the ionization of the residual gas in the space between the electrodes or it might be caused by the electrons knocking positive ions off of the anode. In either case the electrons would have to originate as cold emission electrons from the cathode. If the field necessary for this cold emission was obtained by means of small projections on the surface of the wire, then a layer of gas adsorbed on the wire surface would tend to smooth over these projections and make the potential necessary for the breakdown very much larger. Consequently mercury vapor was allowed to enter the tube by simply removing the liquid air from the trap. The trap was allowed to warm up nearly to room temperature, then the liquid air was replaced and the system was left to stand until the pressure reduced to 10^{-7} mm. of Hg. The 0.5 mil wire was used in this experiment and the breakdown occurred at 10,200 volts. This voltage is in the range of the breakdown voltages obtained with clean wires. Therefore it

can be assumed that the surface was already smooth. From which it follows that the measured field was very nearly the true field. Since from a smooth wire the current would be practically the same from each point of the surface of the wire, the area would be the true geometrical area of the wire. The adsorbed gas would not smooth out projections of large area and it would make projections of small area into hills of large area. Therefore the field would vary by a comparatively small amount from point to point over the surface of the wire. Since the current is an exponential function of the field, the error in the calculated area would be very much greater than the error in the calculated field.

Since the surface is smooth then the fact that the breakdown occurs in the neighborhood of one particular point must be explained by the concentration of the origin of the positive ions. It is easier to conceive of the anode giving off ions from one preferential area than for the residual gas ionizing in one preferential volume. One must therefore conclude that the positive ions are knocked out of the anode.

In this discussion the possibility of the rupture being caused by the electrostatic forces has been omitted. If the rupture had been due to electrostatic forces, the breakdown when mercury was on the wire would have occurred at fields very much lower than when the wire was clean. A stronger argument that the breakdown was not due to electrostatic forces is given by the following experiment.

R. W. Mebs [11] placed resistances in series with the discharge between a point and a plane. He found by microscopic examination of the points after breakdown that for the smaller resistances the rupture was larger than for the larger resistances. This does not, by itself, prove that the rupture was not due to electrostatic forces. The original rupture might have been caused in several ways and then aggravated by the current which flowed from the rupture after the breakdown. The current which flowed would depend on the size of the resistance so that the resulting rupture would depend on the size of the resistance. Therefore the writer tried to place a resistance in series with the central electrode of such a size that the breakdown did not occur.

[11] Mebs, *Phys. Rev.*, **43**, 1058 (1933).

The galvanometer used would indicate a current of 10^{-11} amperes. Therefore a resistance of the order of 10^{12} ohms could be inserted in series with the galvanometer so that as long as the galvanometer did not register, the potential of the guard rings would not be far enough away from the potential of the central cylinder to cause them to lose their effect. Alcohol-Xylene solutions were used for these resistances. The highest resistance was 8×10^{11} ohms. A 0.5 mil wire was used in this experiment. No breakdown occurred when the potential was raised up to 25,000 volts which represents a field of 6×10^6 volts per cm. The resistance was reduced by factors of about three. At each step the potential was raised to 25,000 volts to look for a breakdown. When the resistance was reduced to 3×10^9 ohms, breakdown occurred at 11,500 volts which represents a field of 2.7×10^6 volts per cm. The breakdown had not occurred when a resistance of 8×10^9 ohms was in series with the cylinder. This process was repeated after the wire had again been heated to 2800° K. for 30 minutes. The limits between which the breakdown began to occur were 2.5×10^9 ohms and 7×10^9 ohms.

This criterion for the breakdown is in a way similar to the Kaufman criterion for gaseous discharges.[12] It follows that the breakdown is determined chiefly by the value of the slope of the current voltage characteristic when the current of positive ions is added to the field current. When this slope changes sign, the breakdown occurs.

A further proof that the ions causing the breakdown come from the anode is given by the following experiment. By means of the induction furnace the cylinders were heated to a bright red heat for about an hour. This compares with five minutes heating to a dull red during the evacuation process for the rest of the experiments. No breakdown occurred as the voltage was raised to 25,000 volts within five minutes after the furnace was removed. A 0.5 mil wire was used. After several hours the breakdown occurred at about 11,000 volts. The apparent difference between this and the other experiments was that the cylinder was heated to a higher temperature and for a longer time. Langmuir [13] has shown

[12] W. Kaufman, *Ann. d. Phys.*, IV, 2, p. 158 (1900); *Göttinger Nachrichten*, 1899, p. 243.

[13] I. Langmuir, *J. Am. Chem. Soc.*, **54**, 2798 (1932).

that a surface layer of adsorbed gas forms on a clean tungsten surface in about an hour at a pressure of 10^{-8} mm. of Hg. In this experiment the surface was nickel, in which case the layer should be formed in a somewhat shorter time. Since the only known difference between the two experiments, namely, the one in which breakdown occurred and the one in which breakdown did not occur, was the presence of adsorbed gas on the anode, we can conclude that adsorbed gas on the anode is necessary in order for breakdown to occur.

The following mechanism is suggested to explain this breakdown phenomenon. The heating of the wire to 2800° K. smooths the surface of the wire. This decreases the field to such an extent that measurable currents due to cold emission do not flow. However electron currents below the limit of the measuring apparatus do flow. Considering the electrons as a group, there is a certain number of positive ions formed for each electron which strikes the anode. This number of ions formed per electron can be called the ionization coefficient. The positive ions formed in this way are accelerated through the potential difference between the anode and the cathode. They bombard the cathode with a velocity which depends on the potential applied to the tube.

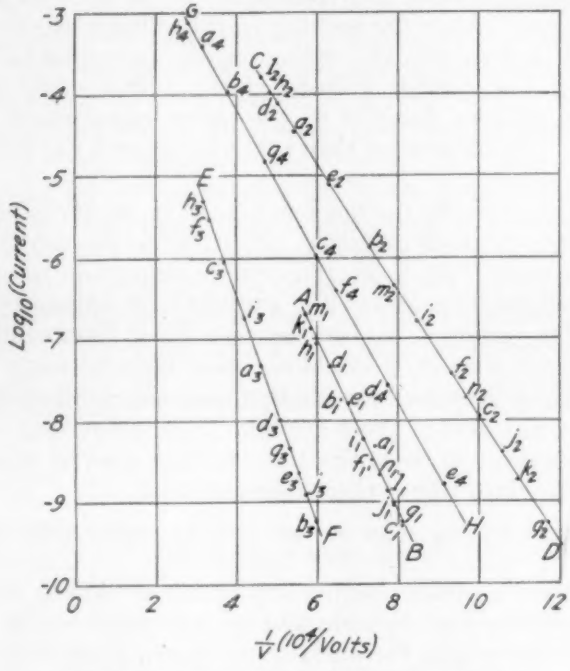
When the potential and the number of ions are both large enough, a rupture of the cathode occurs. Since the binding of the atoms of the adsorbed gas is much less than the binding of the atoms of the anode itself, it is natural to assume that the ionization coefficient is larger when an adsorbed layer is present than when the metal is clean. Such a view would then explain the increased ease of rupture in the case of an adsorbed film of gas as arising from the increase of the ionization coefficient resulting from the film.

4. THE CONSTANCY OF THE CURVES AFTER BREAKDOWN.

Several of the earlier investigators reported that the current was erratic as the voltage was increased for the first time but that the curve obtained as the voltage was decreased was constant and could be repeated at will so long as the maximum current was not exceeded. The writer attempted to show that this erratic character of the curves was due to successive breakdowns.

The following experiment shows that this is the explanation. A wire was placed in the tube and readings were taken before the wire had any heat treatment except the heating by the furnace to 500° C. The results are plotted in Fig. 4.

FIG. 4.



Effects of breakdowns for wires without heat treatment.

The voltage was increased in small steps. Starting with the curve AB readings were taken in the order indicated by the letters a_1, b_1, c_1, etc. No breakdowns were observed while the readings marked by letters with the subscript 1 were made. However, on plotting the readings it was noticed that the readings a_1, b_1, and c_1 fell off the straight line passing through the rest of the points by more than the accuracy of the readings. After making the reading n_1, the voltage was increased until the current and voltage again corresponded to the point m_1. On further increasing the voltage, the current jumped to the value represented by the point a_2 on the curve CD. Read-

ings were then taken in the order indicated by b_2, c_2, d_2, etc. After the reading n_2 the voltage was increased until the current and voltage again corresponded to the point l_2. On further increasing the voltage beyond this point the current jumped to the value represented by the point a_3 on the curve EF. Readings were then taken in the order indicated by b_3, c_3, d_3, etc. After the reading j_3, the voltage was increased until the current and the voltage again corresponded to the point h_3. On further increasing the voltage beyond this point the current jumped to the value represented by the point a_4. Readings were then taken in the order indicated by b_4, c_4, d_4, etc.

In the case of the readings a_1, b_1, and c_1, which did not fall on the line AB, it is probable, according to the remainder of the experiment, that if intermediate readings had been made, the corresponding points in Fig. 4 would have fallen on straight lines. The jumps in current between these lines would have been so small that they would not have been noticed. When the voltage is increased for the first time the number of points falling on any one of these straight lines is not large enough for the observer to recognize the graph as a series of straight lines. The data would then appear erratic.

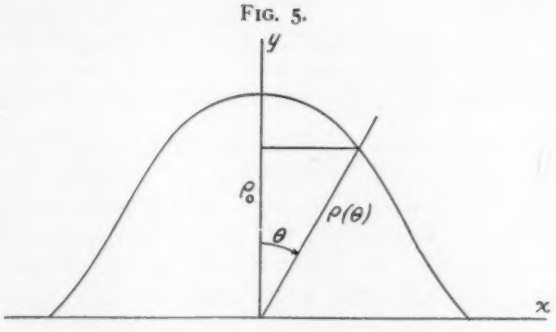
5. THE EFFECT ON THE FORM OF THE CURRENT CURVES WHEN EMISSION COMES FROM A PROJECTION.

In order to obtain further evidence concerning the question of whether the emission can be accounted for by the increase of the field at the surface of a sharp projection, it was thought advisable to find whether or not the variation of the field over the surface of the emitting surface would appreciably vary the form of the Fowler-Nordheim formula for the current. The integration of the current over such a projection can be approximated by simply integrating the Fowler-Nordheim formula over the elemental areas of the projection. This approximation is valid as long as the dimensions of the projection are large compared with the radius of the first Bohr orbit. For in the first place the approximation of the surface potential barrier of the Pauli-Sommerfeld theory loses its validity as we approach the radius of the first Bohr orbit. Secondly, if the dimensions are large compared with this, the elemental areas at the surface of the projection may be considered as infinite plane surfaces.

Thus, for rigor we would have to start by substituting a more exact theory of metallic surfaces than that of Pauli and Sommerfeld. The theory of Block for metallic conduction would be the next approximation. However, to the best of our knowledge no one has obtained surface conditions based on this theory other than the approximation which results in the Pauli-Sommerfeld theory. We therefore make the assumption that the dimensions of the projections are large compared with the first Bohr orbit and allow the problem to degenerate into the integration of the Fowler-Nordheim formula over the surface of the projection.

To get a general expression for the current we simplify the problem by assuming that the projection is cylindrically symetrical about an axis normal to the surface of the cathode. Further, we assume that the function of x represented by the outline of a cross-section through the axis is smooth and single value in x. Using polar coördinants as shown in Fig. 5, the

FIG. 5.



integral of the current over the projection becomes

$$I = \frac{\epsilon}{2\pi h}\frac{\mu^{1/2}}{(\varphi+\mu)\varphi^{1/2}}\int_0^{\pi/2}[F(\theta)]^2 2\pi\rho(\theta)\sin\theta e^{-4/3\kappa\varphi^{3/2}/F(\theta)}\,d\theta. \quad (5)$$

Let $F(\theta) = F_0 f(\theta)$ where F_0 is the value that the field would have at the surface if the projection were not there. Let

$$a = \frac{\epsilon}{2\pi h}\frac{\mu^{1/2}}{(\varphi+\mu)\varphi^{1/2}} 2\pi\rho_0 F_0^2$$

and

$$b = 4/3\,\kappa\,\varphi^{3/2}/F_0. \quad (6)$$

Then Eq. (5) becomes

$$I = a\int_0^{\pi/2} [f(\theta)]^2 \frac{\rho(\theta)}{\rho_0} \sin\theta e^{-b/f(\theta)}\, d\theta. \tag{7}$$

From Fig. 5, it is clear that $f(\theta)$ is a maximum for $\theta = 0$. Let this be $f(0) = \beta$, when $\theta = \pi/2$, $f(\theta)$ is a minimum approaching zero. From the form of the integrand in Eq. (5) and (7), it is clear that zero can be substituted for this minimum without an appreciable error. To simplify the integration let

$$\begin{aligned} f(\theta) &= \beta/y \\ d\theta &= -\frac{\beta dy}{y^2 f'(\theta)}. \end{aligned} \tag{8}$$

Making these substitutions Eq. (7) becomes

$$I = a\beta\int_1^\infty \frac{\rho(\theta)\sin\theta}{\rho_0 f'(\theta)} \frac{e^{-(b/\beta)y}}{y^4}\, dy. \tag{9}$$

If the radius of curvature of the projection is of the same magnitude as ρ_0, $\dfrac{\beta\rho(\theta)\sin\theta}{\rho_0 f'(\theta)}$ is very nearly unity for all values of θ. This assumption prevents us from drawing conclusions from these calculations concerning the emission from sharp projections of the form of a corner of a crystal projecting out of the surface. As pointed out above in cases such as that just described, the Pauli-Sommerfeld theory of metallic surfaces fails. We can only hope that the calculations made here will apply roughly to cases such as these.

It will be shown that in the range of measurements b/β is large compared to unity. Assuming this for the time being, the integral of Eq. (9) reduces to the integral of the exponential factor or

$$I = \frac{a\beta^3}{b} e^{-b/\beta}. \tag{10}$$

Or substituting for a and b from Eq. (6) and putting $F_m = \beta F_0$

$$I = 3/4 \frac{\epsilon\rho_0}{h\kappa} \frac{\mu^{1/2}}{(\varphi+\mu)\varphi^{1/2}} e^{-4/3\kappa\varphi^{3/2}/F_m}, \tag{11}$$

Substituting for these constants the current becomes

$$I = 5.7 \times 10^{-13} \frac{\mu^{1/2}}{(\varphi + \mu)\varphi^2} \rho_0 F_m{}^3 e^{-6.8\times 10^7 \varphi^{3/2}/F_m}. \qquad (12)$$

Thus for all known values of μ and φ, b/β will be large enough so that the approximation of Eq. (10) is valid.

From these calculations we see that if the form of the projection does not differ too greatly from the form of a hemispherical boss, the formula for the current will differ from the Fowler-Nordheim from only by raising the power of the field multiplying the exponential factor from two to three.

In the same manner that Eq. (4) was calculated we can get

$$\rho_0 = \frac{I}{c^1}\left[x\frac{\partial \log I}{\partial x} + 3\right] e^{-[x(\partial \log/\partial x)+3]}, \qquad (13)$$

where

$$c' = 4/3\,\frac{\epsilon\kappa}{h}\,\frac{\mu^{1/2}\varphi}{(\varphi + \mu)}.$$

The calculations of the radius of the emitting area from this formula give values of the same order of magnitude as the dimensions of the effective area calculated by Stern, Gossling, and Fowler.

Since the variation in the form of the new formula from the Fowler-Nordheim formula is of such a character that the difference could not be observed experimentally, we conclude that the explanation of the emission at low calculated fields based on possible projections on the surface becomes still more probable.

5. CONCLUSIONS.

The following conclusions result from the foregoing work:

1. In harmony with the results of previous observations, including those of Del Rosario, the Fowler and Nordheim formula for the relation between the current and the voltage appears to apply as regards its general form, but not as regards the actual magnitude of the field as calculated from the dimensions of the wire.

2. The apparent regularity of the relationships between the current for different wires observed in Del Rosario's experiments is found to be dependent upon the following facts; the conditioning of the wire and the associated vacuum

conditions of Del Rosario's experiments had been pushed to such a point that current did not start until voltages considerably higher than those found by former observers had been attained. It is believed that the actual current from the wire itself had been suppressed by the conditioning to a point at which it was negligible at the instant when the actual current started, this measured current itself coming from an extraneous point on the support.

3. With the wire conditioned at 2800° K., about 700° below the melting point of tungsten, we find that no current flows until after a breakdown and a rupture of the emitting surface even though the field is raised to 6×10^6 volts per cm. We believe that this rupture is due to the cold emission knocking positive ions off the anode which in turn bombard the cathode, the emission current being less than the sensitivity of the apparatus. The voltage at which this breakdown and rupture occurs is reproducible. We conclude that the heating to 2800° K. removes the projections on the wire or at least consistently forms the cathode surface into crystal forms of similar dimensions.

4. When the Fowler-Nordheim formula is used to calculate the current from a projection on the surface of the cathode, no appreciable error is introduced owing to the variation of the field over the surface of the projection provided that the maximum field is considered as covering an area approximately equal to the cross-section of the projection. This conclusion further substantiates the first conclusion.

5. The erratic currents observed by former investigators from a cathode surface which was either newly mounted or moderately heated were due to the successive breakdowns as the voltage was increased.

The investigation was carried out as a thesis for the degree of Doctor of Science in Electrical Engineering under the direction of Prof. W. F. G. Swann, of the Moore School of Electrical Engineering of the University of Pennsylvania and Director of the Bartol Research Foundation of The Franklin Institute. The writer expresses his thanks to Dr. Swann for advice and suggestions and for permission to carry on the work at the Bartol Research Foundation. The writer also wishes to acknowledge the helpful suggestions of Dr. A. Bramley.

HOUSING CONDITIONS IN RELATION TO SCIENTIFIC MACHINE PRODUCTION.*

BY

HENRY WRIGHT,

Housing Consultant, New York.

Although the subject assigned to me is only indirectly in line with my major interest in the technical, social and economic aspects of low cost housing, rather than specifically in the advancement of new methods of construction, however, having addressed myself to the subject I find that I have a considerable store of experience and some knowledge which seems to meet the needs of the situation and promises some possibilities for developing an interesting discussion. My chief concern is a fear that from the abridgment of the title in the announcement some of you may have come expecting quite a different sort of discourse and those most interested in what I have prepared may have stayed away—for while "Housing Conditions, etc." as a title gives me an unusually wide latitude it could not very well have had much selective value in determining the audience and leaves me somewhat uncertain as to the reception which my subject matter may receive.

You must be fairly well aware that the fate of the construction industry is one of the central elements in the problem of industrial recovery. This industry normally furnishes about 10 per cent. of all industrial employment. In past depressions it has had a tendency to lag behind other forms of activity both in reacting to a downward financial trend and in recovering at the end of a depression period. Its behavior in the present instance is complicated by the fact that a strong dose of over expansion in the upswing of 1921 to 26 caused it to start a retrogressive movement three years before the financial crash of 1929. I cannot quote the figures for the industry as a whole but in 1926 the construction of

* Presented at the Stated Meeting held Wednesday, January 17, 1934.

dwellings reached a peak of 3 billion dollars for the year's output representing 50 per cent. of the entire field for that year while by 1929 this type of construction, omitting New York City, had fallen off to half this amount in dollars and somewhat less in units produced. In New York the decline came later with the spectacular result that certain very large building projects including apartment houses which, having required a lapse of 18 months from inception to the actual start of construction, were going merrily up when similar surrounding buildings completed a year or two earlier were standing with only a handful of tenants. It may be of interest to add that in New York the amount of housing for 1928 and 1929 was approximately the same in total dollars for Manhattan and the Bronx, but changed from one-third in Manhattan and two-thirds in the Bronx to two-thirds in Manhattan and one-third in the Bronx, with a decided shift to much higher cost construction.

Another matter worthy of note is that in the peak year of 1926 in which 3 billion dollars worth of residential construction was recorded it represented 500,000 dwelling units, and since 120,000,000 people at an average of five per household require about 24,000,000 dwellings, we were in that year building at a rate which if continued for 48 years would replace all the dwellings in the United States, and would still further intensify the problem of deciding what to do with all the old dwellings on hand.

The building industry has been very hard hit, particularly in cities such as Detroit and Cleveland. In the latter city, 1933 construction was only about 5 per cent. of that of 1927 or 28. It is difficult to predict whether the industry as a whole has any chance to come back to its prosperity proportions. For the present at least there would seem small chance of much activity in Industrial or Commercial Buildings. Dwelling construction is its most promising hope, and since in the high class field we temporarily at least exhausted the market in the ten years of post-war activity, it is fairly well conceded that some form of moderately low cost housing must take the burden of building output. Here the presence of very considerable vacancies and the shrinkage of purchasing ability would seem to offer insurmountable odds were it not that we

have accumulated a very real shortage in accommodations in relation to actual if not realizable needs. A five year holiday in building, equal to that of the war period has already taken place, the potential shortage has been further augmented by a widespread tendency to raze unoccupied buildings so as to save taxes and by the yearly disappearance of houses in slum areas of frame construction to provide fuel for the neighbors.

We may also add the fact that our general housing plant is perhaps relatively more backward than almost any other field; new and modern educational, church and office structures and especially industrial buildings have been not only more advanced quantitatively but in respect to their modernization. New industry has demanded completely new equipment including the roof over its head. People have remained just people and have been content with a house of the old buggy days except for a few meagre additions of convenience gadgets worked in at odd places.

In preparing its series of articles published from January to June in 1932, *Fortune Magazine* (reprinted in book form as "Housing America"—Harcourt Brace & Co., publishers) concluded that not more than 50 per cent. of the population of America enjoyed anything like decent modern accommodations or more than the rudiments of sanitation and conveniences.

More than 500,000 of New York's original old-law tenements built 50 to 80 years ago are still standing, though their rate of vacancy at this time is very considerable. These dwellings were virtually outlawed by the first tenement house code of 1901.

The Committee of Large Scale Operations, Presidents Housing Conference of 1931 stated that "While the standards of motor cars, radios, etc., have been transformed in the last decade . . . the houses of the country constitute our largest mass of obsolete and discredited equipment."

I am convinced that part of this great discrepancy between what people have and what they might have in the way of decent modern dwellings lies at the door of the building industry, in its lack of imagination and its traditional conservatism. In this day and age what people want and find means of having is to no small degree conditioned by what they

are induced to want. Cafeteria style may be all right for restaurants but in housing and particularly low cost housing the industry has an undischarged obligation in finding something both attractive and palatable and in placing it before our people in consumable shape. It is for this reason that I have coupled the fate of "Scientific Machine Production" in building with the program of Slum Clearance and Low Cost Urban Housing rather than with its more popular aspects of demountable houses for the suburbanite. It's anybody's guess whether we are about to so completely decentralize as to leave our older cities with their antiquated housing equipment to rot in their tracks, or whether business necessity is going to stage a big comeback in the rehabilitation of our blighted cities. While I am traditionally on the side of the decentralizers, I am not sure but there may be strong points in favor of saving the credit of our good old cities however much they may deserve to be numbered among the forgotten and deserted of the earth. It is probably certain that we can't go on doing a whole lot of both at least for the present or perhaps for some time to come.

Some of you may have wondered what happened to all the hue and cry about prefabricated houses to be turned out in endless belt automobile fashion and all ready to sell over the counter at $3000 to $5000 complete. This eventuality seemed startlingly imminent about midsummer 1932 and then faded out like a meteor. There are probably many phases of the situation which I do not grasp, but at least one was the conclusion that while industry was ready to turn them out people were in no shape to buy them at even the remarkable announced prices, to say nothing of what they would really have cost anchored down to a town lot and public utilities.

A new house recently built for sale in the New York suburban area on the basis of one of the prefabricated designs such as those promised for approximately $5000 is priced at $20,000. Naturally under present conditions no benefits of large scale production can be reached but even so the spread between $5000 and $20,000 while entirely understandable is not altogether encouraging.

At any rate, as fascinating and popular as that subject may be, I am going to pass it by with a very few observations, since for the present it has become to my mind rather academic. However, if houses are to be manufactured their production is legitimate bait for the great over-equipped automotive industry. Except for the difficulties of getting by building codes and trades practices they are in fact all set ready to go from a practical standpoint.

Mr. Kettering, engineer of the General Motors, in an article in the *American Magazine* about 1928, assured his readers that engineers were entirely ready to develop a practical house at a reasonable cost on a purely scientific basis, but that it would be useless to do so since the public would not accept a house embodying the radical departures which would be properly and unavoidably incorporated.

But if it is their job I don't see eye to eye with those who wish to fabricate a line of identical complete prefabricated houses. Manufacturing interests are concerned primarily with production, rather than salesmanship. The selling of houses is a most complicated procedure, and includes dealings with the city authorities, making connections to utilities, and many other matters extending over a long period of time, which are by no means comparable to the Automobile Acceptance Corporation. It seems to me a better bet for industry for the present to limit itself to the production of modernized, compact, complete and fully practicable mechanical units for houses and leave the walls, windows and book shelves for the other fellow. The present director of research of the Pierce Foundation developed this idea and presented it to one of the leading Automotive Engineers in 1927. The Engineer was greatly interested and regarded the plan as a logical expansion of the industry, but automobiles were selling fast at the time and the higher ups demurred. The idea was that of building a prefabricated mechanical unit of truck transport size combining the working parts of the Bath Room, Kitchen and a Fireplace heater on the two sides and one end of a prefabricated entity about 6×9 feet in size. The scheme was by no means abandoned, however, and has since been developed in laboratory form so that for two years an experimental unit of this kind has been in place and in

daily use in a completely modernized house erected on the top of a 19 story New York industrial building. To conduct you through that little house would be like Pandora lifting the lid off a thousand evil spirits. I don't know how evil those really were, but though a completely new, compact, dependable, fully integrated mechanical unit of this kind may not seem like an evil spirit, if it were let loose tomorrow it would cause such a ruction in the building world that we would be convinced that the revolution had really arrived. Let me give just one illustration—the bath room and kitchen in this unit, has been newly developed throughout, and subjected to strenuous testing. Its piping occupies less space and weighs about 50 per cent. of the usual job "rough in," the fixtures have all been developed for their specific use in the complete assembly. Altogether as it stands even in respect to appearance it is as startling as the new Chrysler Car in contrast with the old. It seems to be equally remarkable in effectiveness and absence of the usual noises and stoppages. I am unfortunately not even permitted to describe the combined open fireplace and heating unit in this house or its unique heat distribution. But what of it? How ready are we for a real advance in even such a Godsend as a sensible dependable bathroom would be? Let us see. I live 50 miles west of New York in a sparsely settled part of New Jersey. When I want to do a job of plumbing I go to the hardware store and select any desired length of pipe nipple and other necessary parts. Such nipples cost from 12 to 20 cents each. Were I to employ a plumber he could not use the nipple even if I supplied it. He has to take a full piece of pipe threaded perhaps at one end, cut it off, and make the necessary new threading absolutely at the job. The time and waste would cost me at least 5 times the cost of the store article. Imagine such a plumber installing a prefabricated "rough in" for a whole bathroom, or admitting the fact that it is twice as good a job when completed.

Now it is things like this which lead me to believe that we're not quite ready for the completely prefabricated house to be sold in the open market and dependent on installation by existing trades. On the other hand these same things lead me to predict that the days of hand crafts in building are

unfortunately numbered and the time is about upon us when certain ponderous inevitables will overcome certain imponderable old fogeyisms.

Without belaboring the point or tempting Pandora further in regard to the future house of her dreams, let us pass to what to me is a more vital problem though less spectacular—the more fundamental structural changes and improvements available primarily for the use of urban housing. Perhaps I should pause here to make a clean breast of my idea of good urban housing. I know that Philadelphia feels that it has been entrusted with the Holy Grail of low cost housing and can show the world more ways of taking what might be a simple row house and distorting it into odd shapes and appearances than man has ever known. *I'm all for the row house*—that is a good row house in its proper place, but unlike Philadelphians I don't insist on *Row Houses for All.* I don't even think that it was decreed by Angel Gabriel that every decent citizen below say a $5000 per year income should live in a row house if he expects to get to heaven. However, I'm not so far from agreeing with you at that. I'm heart and soul for low dwellings and not for tenements of 4 or 5 stories, such as are being too often accepted as necessary for low cost housing especially when trying to use high cost land. My preferences include a somewhat wider interpretation of the row house and I'm particularly antagonistic to too much "rowiness." To my mind a reasonable form of dwelling for most urban areas and covering the widest field of usefulness is yet to be developed or rather evolved. It may be described as the "Group House" to which much space is devoted in a book I have recently completed. It is a building of somewhat irregular outline, generally of three stories but often two and less frequently four, divided either by insulated floors or by insulated walls into a variety of housing units. Frequently these units occupy two separate stories but quite as frequently they may be horizontally arranged. Complete privacy can and must be obtained and usually but not necessarily always in respect even to their stairway entrances. A few units with common entrance stairs may be found desirable. Such dwellings may be homelike and domestic to whatever extent you insist, though under some conditions of

necessarily restricted land area, complete exemption from stray cooking odors and radio noises may not at present be accomplished. But these are present also in closely built so-called single family houses and await correction through new improvements in ventilation and in habits of common courtesy rather than adherence to old-fashioned house forms. You may ask what kind of a dwelling is it—"a single family or a multi-family house or an apartment"? My answer is "I don't know and don't care." All these distinctions are rapidly disappearing. All forms of good housing are tending more or less toward uniformity in characteristics and standards of convenience. The house has come to resemble the apartment in its automatic sufficiency while the apartment endeavors to supply homelike quality and individuality. To me *good housing* is suitably arranged living space properly related to sunlight, air, outlook and community amenities; what you may call it is to my mind of minor importance. Therefore, admitting for the sake of argument at least that such is the city swelling of the future, what aspects of improved construction and machine production can be applied to its satisfactory development?

Suggestions for new forms of construction have flooded the building industry for the last fifteen years—most of them have had to do with new kinds of wall construction though floors have come in for their share, but it is only recently that the two have been brought together on anything like an amicable basis. Every new idea of this kind has been calculated by its originator to revolutionize the building industry but too many have failed to help housing very much because they tried to be all things to all kinds of buildings. To try to be all things to at least one house, was characteristic of the rather doubtful new kinds of houses displayed at the recent Chicago Fair, where each of a half dozen kinds of manufacturing industries, attempted, unsuccessfully to my mind, to show that you could build a complete house out of one or another material such as brick, wood, pulp board, metal or what not. We need a Walter Disney to make a comic such as—"I build my house of stone, I build my house of bricks, I don't give a hoot what the other fellow does for business ideas won't mix."

The fundamental failure in all this 15 years of effort lies in one unfortunate assumption (here were reviewed some 20 kinds of new forms of construction, mostly for walls only, developed from 1915 to 1930), they were all directed to an ancient conception of building, one in which walls were erected to rest floors on; when a new science of building was evolving ways, already applied in industrial and commercial building, in which floors are erected to rest walls on.

Perhaps the biggest impetus to this problem was initiated when the Government wrote into its current Housing Program a differential in amortization rates which makes fire resistant construction almost imperative. Architects first interpreted this as fireproof, in ordinary "code" terms, and I think the Housing Division itself failed to realize that by applying a new requirement to the field of housing they were destined to initiate an entirely new kind of construction, one in which fireproof methods freed from code restrictions applying to heavy fireproofing were to be advanced into a new field of light construction requirements. Even so it is not light steel beams of makeshift industrial floor methods that seem about to emerge, but a simplified reinforced slab and column construction adjusted to the light load requirements and short spans of efficient two room deep dwelling space. With the danger of being too technical I want to show how the thing evolved or is in the act of evolving at the moment. Warehouse or office building construction has largely employed reinforced slabs and mushroom head columns. But here a number of internal bays caused a natural cantilever action from post to post, until the last or "wall bay" was reached to which special things had to be done, costly but only involving a small area. In the two room deep dwelling all the bays are "wall bays" so the problem is quite different. This problem requires specific design of new column and slab spacing either with wall posts or, as some engineers are suggesting as the most efficient and cheapest method, the principle of cantilever overhang, which has been successfully used for a number of years in low cost housing, particularly in Holland. Now I can't go on here trying to give a lecture in engineering; the important lesson I want to convey is that dwelling construction constitutes a special problem which

hasn't been important enough heretofore to waste any time or talent on. Now that the dwelling is in the limelight, it's getting a treatment all its own, and this is already doing remarkable new things to old kinds of "new" construction.

My own direct acquaintance with the problem is about 60 days behind the advance guard so I can't say where we are today—but engineers acting independently in three cities seemed then to be converging to a common point—at least to an independent post and floor construction of the simplest kind of light reinforced concrete slab of uniform thickness and without beams or splay column heads, leaving the freest possible space to fill in with both outside walls and inside partitions. While this may be too technical to be easily grasped, what is most important to understand is that it renders obsolete 15 years of effort to invent new wall methods which were each going to revolutionize the industry and solve the housing problem—walls which tried to straddle two contradictory things, support and shelter. If we are now on the right track, walls will insulate and divide but no longer support, and prefabrication at least of wall enclosures will become eminently logical, leaving basic structure only to be formed on the job.

To return to the new structure independent of outside walls and inside partitions. This is a real major gain in housing as I see it. Two things have irked me about usual housing design for some time, one was the rigidity of the plan, the other the limitations of the architectural window. I'm not an advocate of the modern versus traditional in architecture. In fact I'm not very ardent about architecture in general as a prime matter in the inevitable period of evolution. I like to know how people live in the buildings after they are designed and built and I find people chafing at narrow room sizes, lack of furniture space, and limitations of outlook. As much as I may yet incline to approve the external appearance of wall piers and symmetrical window spacing I find myself wanting to kick out the pier that centers just where I want to see out of the end of my living room. After all people live *in* houses not on the outside. So I'm enthusiastic about a type of construction which leaves things as flexible as possible. Naturally the stairs and plumbing lines have to be fairly solidly clamped down, otherwise I should like to see

a given dwelling space worked out fifty different ways in the same space rather than one way in fifty spaces. I was particularly gratified to study a year ago at Stuttgart an early development of this idea, even under construction difficulties of old wood joist pattern in the apartment of Meis van der Rohe at the Weissenhoff Siedlung. He is one of the pioneer advocates of building horizontal space supported most any way to keep the floors level and solid, and filled in *ad-lib*, the outside window location and spacing to be left to the last. I can see no particular need of a great expanse of glass but I do feel that our design must permit openings to be placed so as to afford the best window positions and shapes for outlook and lighting and sun penetration. The thin non-supporting outside wall with grouped windows and no reveal, is admittedly a difficult but not insolvable architectural problem. The results in increased sunlight are surely worth the effort.

But I could go on almost indefinitely with illustrations of this kind and you would have an entirely false impression of me. I'm only indirectly interested in such details as I've been giving you, except as they get us on our way toward a sensible attitude to the vast problems of the moment. What are we going to do with our blighted cities, how are we going to break down our sentimental attitude toward ownership, how are we going to show narrow-minded and selfish people that holding on to property which has long passed its usefulness and is really worthless in its present obsolete and depreciated state is not only anti-social, it is spelling disaster alike for the owner and his community?

We must somehow bring about a realization of a changed world, changes which have nothing specially to do with the "New Deal," but with the fact that we are entering a period of stabilized population where cities won't just grow and fill the pockets of property owners with profits, but where cities will get out and compete in good service to attract population, and when it will no longer be genteel or even shrewd to be either the private or public possessor of slums. I like to see flexibility brought about in planning because it may bring with it some flexibility in the minds of planners. I want to see us break away from tradition not alone in design, but in our

belief that we can bottle up people in narrow places, when a little care will give them unlimited sunlight and freedom.

To many of you what I have been discussing may have for the most part seemed entirely technical—and while I hope understandable, may not have reached into the subject along lines which touch upon your special interest. I shall assume that you are, however, interested in some of the implications of the inevitable major changes which are logically likely to take place. You have felt at least vaguely that something was more or less imminent in the matter of new and better forms of house construction, of if not better at least sufficiently odd to cause you to believe that it is necessary for a well informed person to try to keep reasonably abreast of them. Let me outline to you my ideas of the long term aspects of the situation so as to suggest at least some basis for a comfortable layman's attitude to the subject. Changes you may be sure are certain to take place which will vitally alter not only the form and construction of the house but also the conditions by which it is owned and occupied. The largest room in any present house is room for improvement and if we use the development of the automobile as a parallel, we have scarcely scratched the surface. One reason that air conditioning which in itself is directed to no very well defined purpose, is at least a significant matter, is that it begins to introduce a really scientific note into the broad problem of heat and ventilation. My feeling is that we know about as much fundamentally of these matters as the airplane builder did when his wings were like the flat sides of a box kite. Later we found that the plane was not buoyed up from below but sucked up from above. Eventual house air conditioning far from a mere supplying of hot or cold air may eventually carry us as far from our present concept of the whole house as the new airplane wing in contrast with the old. This is only an example. The house under far more complex conditions must for many years run the gamut of "cut and try"—adding this, adding that,—limited by the waste of business competition acting through the unstable whims of the popular mind. This period may last even longer than with the automobile before we get down to vital matters of living comfort and dependability. Don't get too excited

during this stage and believe everything you hear or see that is startling is really important. Some day we will enter that happy place in housing that to some extent has been approached by the automotive designers where real science even under competition is beginning to center on comfort, roadability, dependability and sweetness of action. Then indeed we may all sit up and take notice, for soon we shall have dwellings which are really livable and abreast of what science and the arts are capable of producing.

Element Ninety-Three.—(*Chemistry and Industry*, Vol. 53, p. 678.) The fields of chemistry and atomic physics are due for some new excitement, if it has not arrived already. Up until recently it has been tacitly admitted by most self-respecting scientists that there are only 92 elements—no more, no less. As if to shatter such complacency, an Italian scientist, Dr. Enrico Fermi, deliberately shot neutrons at uranium (element 92) and believed that some stuck in the nucleus, thus creating element 93. This announcement has naturally been received with some scepticism in other scientific quarters and refutations have begun to appear.

Now to make matters more interesting, just about the time doubts were being cast upon Dr. Fermi's synthesis of this new element, Odolen Kablic, a Czech engineer, announces the discovery of the new element Bohemium found in a natural state, associated with pitch blende, the well-known uranium ore. Apparently the existence of another element heavier than uranium has a logical basis, for scientists have never been satisfied that uranium was the progenitor for proto-actinium. The chemical behavior of this new substance corresponds to what the periodic table predicts for element 93. The atomic weight appears to be approximately 240, another confirmation of the element's numerical identity. The pitch blende of Joachimsthal seems to contain 1 per cent. of it. The mean life of this radio-active element is estimated at perhaps 500,000,000 years. Prof. Dolejsek of the University of Prague is attempting to obtain its x-ray spectrum.

C.

NOTES FROM THE U. S. BUREAU OF STANDARDS.*

MEETING OF INTERNATIONAL RADIO CONSULTING COMMITTEE IN LISBON AND OF THE INTERNATIONAL SCIENTIFIC RADIO UNION IN LONDON.

On recommendation of the Department of State, Dr. J. H. Dellinger, chief of the radio section of the National Bureau of Standards, has been appointed chairman of the American delegation to the International Radio Consulting Committee (C. C. I. R.) which meets at Lisbon, Portugal, from September 22 to October 10, 1934. The other official delegates are Major Roger B. Colton, Signal Corps, United States Army, C. C. Gross, Federal Communications Commission, Captain Stanford C. Hooper, United States Navy, and W. Vallie Whittington, Treaty Division, Department of State. The delegation will also include representatives of the American Telephone and Telegraph Company, American Radio News Corporation, International Amateur Radio Union, Mackay Radio and Telegraph Company, National Association of Broadcasters, and RCA Communications, Inc.

The International Radio Consulting Committee was established by the International Radio Conference of Washington, 1927, and reëstablished by the International Telecommunications Conference of Madrid, 1932, to advise the radio administrations of the world on technical questions. The general objective of its work is the reduction of radio interference.

This Committee has had two previous meetings at which a large number of questions were studied, leading to 50 Opinions on a wide variety of technical radio problems. These Opinions do not have the force of regulations, but they are accepted by all governments as the authoritative expression of the status of technical progress.

The subjects to be discussed at the Lisbon meeting are as follows:

* Communicated by the Director.

1. Organization regulations of the C. C. I. R.
2. Participation of international organizations in the C. C. I. R.
3. Working of a mobile station accurately on the frequency of land station.
4. Technical basis of allocation of bands of frequencies.
5. Mitigation of harmonics in radio transmission.
6. Reduction of electrical interference.
7. Selectivity and frequency stability of radio receivers.
8. High-frequency calling frequencies.
9. Modulated telegraph transmission.
10. Mitigation of key clicks in radio telegraph transmission.
11. Standard frequency transmissions.
12. Measurement of telephone noise and voice levels.
13. Radio telephony between small ships and land stations.
14. Telephony with moving trains.
15. Coördination of fixed-station radio telephony and wire telephone system.
16. Methods of measuring radio field intensity and noise.
17. Synchronization of broadcast stations.
18. Frequency separation between broadcast stations.
19. Efficacy of directive antennas (150 to 1,500 kc/s).
20. Efficacy of "anti-fading" antennas.
21. Broadcasting by single sideband.
22. Study of wave-propagation curves.
23. Measurement of transmitter spectra.
24. Revision of earlier C. C. I. R. opinions.
25. Characteristics of arc transmitters.

Dr. Dellinger will also attend a session of the International Scientific Radio Union (U. R. S. I.) in London, September 11 to 19. This is one of the Unions in the International Council of Scientific Unions in which this country is represented through the National Research Council. The London session will include meetings of technical committees dealing with Measurements and Standards, Propagation of Waves, Atmospherics, and Radio Physics. For these meetings the Bureau has prepared the following special reports: Radio frequency standards; An analysis of continuous records of field intensity at broadcast frequencies; Frequency distribu-

tion of the intensities of radio atmospherics; Report on Polar Year observations.

GASOLINE KNOCK RATING.

The relative tendency of gasolines to knock or detonate in internal combustion engines is evaluated by comparing them in a standard test engine with blends of two pure hydrocarbons: *normal heptane*, which knocks more readily than any gasoline, and *iso-octane*, which has less tendency to knock than any commercial gasoline. The percentage of iso-octane which must be blended with normal heptane to match a given gasoline in the knock-test engine is known as the octane number of the gasoline.

Fuels for automobile engines are rated according to ASTM method D357–33T, see Method 600.1 in Federal Specification VV-L-791 (Methods of Sampling and Testing Lubricants and Liquid Fuels) which can be obtained from the Superintendent of Documents, Government Printing Office, Washington, D. C., for 15 cents a copy. The test engine and procedure specified were developed over a period of years by the CFR Sub-Committee on Methods of Measuring Detonation, which comprises representatives of leading automotive and petroleum laboratories and of the National Bureau of Standards. An extended series of coöperative road tests, to ascertain whether the ASTM octane numbers thus determined are in satisfactory agreement with the relative performance of representative motor fuels in the 1934 automobiles, was concluded on August 7 and the results are now being analyzed by the subcommittee. Most competitive-price gasolines today have knock ratings between 65 and 70 octane number. Gasolines of higher knock rating usually command a premium while the cheaper grade 3 gasolines may have ratings as low as 50 octane number. The grade of fuel required by any particular automobile depends on engine condition and type of service as well as on engine design.

The Coöperative Fuel Research Committee is also sponsoring the development of a standard method for rating aviation gasolines but, pending the outcome of this work, the ASTM motor fuel method is used by the Department of Commerce and the aeronautic industry for rating commercial aviation

gasolines. The National Bureau of Standards has recently completed tests of 26 aviation gasolines in a 9-cylinder Wasp engine, capable of developing over 600 horsepower at rated speed. These gasolines included three reference fuels and four groups of test fuels. The test fuels ranged in octane number from 69 to 89 and each group represented a distinct type of commercially available fuel. The object of these tests was to compare the behavior of these diverse types of test fuels with the reference fuels in a particular multi-cylinder aircraft engine under conditions of incipient detonation. Similar tests are being run by a number of engine manufacturers using other representative commerical aircraft engines. The results of all these tests will show whether any changes in the laboratory test procedure are required to obtain octane number ratings of aviation gasolines which agree with their average performance in modern aircraft engines. The fuel requirements of such engines range from below 70 to at least 87 octane number.

MEDALS AWARDED BUREAU SCIENTISTS BY FRENCH SOCIETY FOR WORK ON PHOTOGRAPHIC EMULSIONS.

At the meeting of the French Society of Photography and Cinematography held on January 26, 1934, it was unanimously voted to award the silver medal of the Society to Doctors Burt H. Carroll and Donald Hubbard as testimony of the importance which it attached to the work done by these members of the Bureau's staff while engaged in research on photographic emulsions. The report which recommended the award stated that the work of these two men in this field has been of great benefit to French investigators and industrialists. The Bureau greatly appreciates the action taken by the French Society in thus honoring two members of its staff and for expressing its commendation of this work, which, due to lack of funds, it has been impossible to continue.

CARBON PAPER AND TYPEWRITER RIBBONS.

Every user of carbon paper and typewriter ribbons wishes to get the best and longest service from them. The National Bureau of Standards has prepared a mimeographed Letter

Circular, in which it is shown how anybody can make writing tests of carbon paper and typewriter ribbons, and select for himself the kinds that are best adapted to his special needs. Brief accounts are also given of the manufacture of these materials, and it is pointed out how their physical properties and their composition are related to their writing qualities.

When carbon paper is to be used for making only a small number, say up to three, copies at one time, the most important test is serviceability, or wear-down. This consists in writing the same letters over exactly the same part of the carbon paper, about 15 times, or until the coating is worn off. The best paper will write the greatest number of lines that are of good intensity of color, and sharp enough to be read easily.

If it is desired to make a considerable number of carbon copies at one time, the manifolding test is made. In this, five sheets of the carbon paper, with an ordinary first sheet and five thin copy sheets, are used. The numerals and the entire alphabet, both small letters and capitals, are written twice over. All the numerals of the fifth copy should be legible, and not more than five of the 104 letters should be so indistinct as not to be recognizable.

When making these tests, the first, or ribbon, sheet and the thin copy sheets should be identical with those to be used in actual service. The character of the carbon copies depends not only upon the hardness of coating and other properties of the carbon paper, but also upon the thickness and stiffness of the first and the copy sheets, the size of the type, the hardness of the rubber-covered platen, the touch of the operator, and other factors.

Typewriter ribbons are given a serviceability, or wear-down, test similar to that for carbon paper, but 100 lines are written. Then, after allowing the ribbon to rest undisturbed for a definite time, another line is written, in order to determine the recovery, or flow of ink in the fabric. Another test, to find out whether the ribbon has a tendency to clog the type, is made by writing the letter e 800 times, when the ribbon is moving in the usual way, and not kept still as in the serviceability test.

A few other determinations, which can not be made without laboratory facilities, are briefly discussed. They are

useful in specifications, but do not have such a direct bearing upon the writing qualities of carbon paper and typewriter ribbons as the test outlined here.

A copy of this Letter Circular No. 424 will be furnished on request.

STANDARDS FOR CHECKING OPACITY METERS.

Opacity of paper is commonly specified by the contrast-ratio method. According to this method the reflectance of the material backed by a perfectly absorbing surface divided by its reflectance when backed by a highly reflecting material such as magnesium oxide, is taken as an index of opacity. Magnesium oxide, itself, is, however, commonly not used as the white backing because of its fragility; nor is the white backing placed in actual contact with the sample. Because of these and other sources of error, opacimeters frequently give erroneous results.

Glass standards for checking the accuracy of opacity readings when testing paper and other sheet materials such as tracing cloth, transparent wrapping papers, envelope paper, envelope windows, and particularly printing papers, can now be obtained from the National Bureau of Standards. As a result of the study reported in Research Paper no. 660, "Sources of error in measuring opacity of paper by the contrast-ratio method" (*B. S. J. Research*, March 1934), it has also been found that opacity as determined by the Technical Association of the Pulp and Paper Industry method differs appreciably (for some papers by as much as 0.04 or 4 per cent.) from true contrast ratio defined in Bureau of Standards Circular no. 63.

To provide a means of checking the adjustment of opacity meters of the type described in Circular no. 63, to discover whether they are reading according to the TAPPI definition, opacity standards of opal glass have been developed. These standards are 5 × 20 cm. rectangles, 1.5 mm. thick, and are permanent and cleanable. The theory of their application is given, and the results of tests in which they were used are reported in RP709 in the September number of the *Journal of Research*. The opacities of these standards, according to the TAPPI method, fall between 80 and 90 per cent. (0.80

and 0.90 contrast ratio), which is the critical range for printing papers. A standard may be purchased from the Bureau for $15.00 or may be borrowed for one week free of charge.

A copy of Letter Circular no. 418 which contains detailed instructions for the use of these standards, may be obtained on request to the National Bureau of Standards. Copies of Research Papers nos. 660 and 709, and of Circular no. 63 are obtainable only by purchase at 5 cents each from the Superintendent of Documents, Government Printing Office, Washington, D. C.

ZINC PHOSPHATE DENTAL CEMENTS.

As part of the coöperative research on dental materials conducted at this Bureau by the Federal Government and the American Dental Association an investigation of the physical and chemical properties of zinc phosphate dental cements has just been completed. A preliminary report of this investigation was published in the November 1933 issue of the *Journal of the American Dental Association*. The final report will appear in the November 1934 issue of the same journal.

Seventeen trade brands of cement were studied. Test methods and apparatus were developed to determine composition, consistency, setting time, crushing strength, dimensional change on setting, disintegration, and film thickness. Studies were also made on their hydraulic, adhesive, and other properties which might be considered significant in dental usage.

The major constituent of all powders, except one, was zinc oxide. This is sometimes modified, up to 13.0 per cent., with magnesium oxide, or up to 4.5 per cent. with bismuth oxide. The liquids were phosphoric acid buffered with zinc oxide and aluminum hydroxide. The consistency was determined by measuring the diameter of a disk of cement formed by pressing a definite volume of soft unset cement (0.5 c.c.) under a load (120 g.) between two glass plates for 10 minutes. In the preliminary report the proportion of 1 g. of powder to 0.5 c.c. of liquid was used. The setting time at mouth temperature varied from 5 to 15 minutes, and at 70° F. (65 per cent. humidity) from 5 to 40 minutes. The crushing strength varied from 3,500 to 16,500 lb. per in.^{2} when stored in water for 24 hours. All cements studied disintegrated somewhat when placed in

water for six months. All shrank appreciably on setting when measured on a dental interferometer imersed in water. The properties of these cements were affected markedly by temperature and slightly by humidity changes. Adhesion, if the term may properly be applied to these cements, was found to be entirely mechanical in the set cement. No cement was truly hydraulic as all showed increased deterioration if exposed to water during setting. After 7 days immersion in distilled water a disintegration of from 0.1 per cent. to over 10 per cent. by weight was noted. Manifestly those cements showing large percentage disintegration are inferior materials.

A tentative specification was formulated by the Bureau's dental laboratory. This specification was submitted to manufacturers, dental schools, and a large committee of dentists for criticism and suggestions for improvement. It was finally submitted to the American Dental Association in St. Paul on August 10 for consideration. The specification was adopted as A. D. A. Tentative Specification No. 8 for Dental Cementing Medium to become effective July 1, 1935. This will be used as a standard of quality for dentists who use this material. The following is a summary of the Detail Requirements:

Consistency of Mix.	Time of Setting at 99° F.		Ultimate Compressive Strength.		Film Thickness.	Disintegration.
	min.	max.	min.	min.	max.	max.
	minutes	minutes	one hour	7 days	microns	per cent. by weight
Disk 30 ± 1 mm. in diameter	4	10	350 kg./cm.² (5000 lb./in.²)	840 kg./cm.² (12,000 lb./in.²)	50	1.0

PROPOSED CHANGE IN THE METHOD FOR MEASURING THE ACIDITY OF SOLE LEATHER.

During the past few years the National Bureau of Standards has been conducting researches relative to the effects of sulphuric acid on the life and properties of heavy leathers. It has been found that the deterioration of vegetable-tanned

leather due to sulphuric acid starts near pH 3 independent of the kind of tanning material used, the aging conditions, the degree of tannage, and the amount of acid in the leather. Recently results of aging tests on leathers treated with oxalic acid have been completed and the same tendency for deterioration to start near pH 3 was observed. The advice and assistance of an advisory committee, which jointly represented the trade and technical interests in the industry, were had in this work.

These two acids are the ones commonly used in the manufacture of commercial sole leather. The results of the research make it appear that a pH measurement is a better criterion of the harmful acidity than the present Proctor and Searle method for the following reasons:

1. The above described research which shows that the leathers containing both acids start to deteriorate at about the same pH regardless of the acid content.
2. The Proctor and Searle method does not show the presence of oxalic acid.
3. The amount of sulphuric acid required to deteriorate leather varies with a number of conditions. In this work the amount present varied from about 0.3 per cent. to 1.8 per cent. at pH 3. In some commercial leathers it runs as high as 2.3 per cent., as determined by the Proctor and Searle method, although the leathers are satisfactory with respect to harmful acidity.

In the case of sole leather, which is usually consumed within a short period, it is believed that a minimum pH limit of 2.75 will insure the procurement of satisfactory material so far as acidity characteristics are concerned. It is proposed to revise the Federal specification for sole leather by substituting this limit in place of the present one per cent. limitation as determined by the Proctor and Searle method. The revision has been approved by the Committee on Leather Products of the Federal Specifications Board and the matter has now been presented to interested industrial groups for consideration.

THE SYSTEM $CaO–B_2O_3–SiO_2$.

To determine the effect of boric oxide upon the melting points and temperatures of formation of the calcium silicates contained in portland cement, namely, dicalcium silicate and tricalcium silicate, a study of the ternary system $CaO–B_2O_3–SiO_2$ is in progress. The binary system $CaO–B_2O_3$ has already been investigated by E. T. Carlson (*B. S. J. Rsearch*, 9, 825 (1932); RP510), and that of $CaO–SiO_2$ by Shepherd, Rankin, and Wright (Am. J. Sci., (4) 22, 265 (1906)). In working out the fields of $3CaO.SiO_2(C_3S)$ and of $2CaO.SiO_2$. (C_2S) in the ternary system it was found necessary to extend the investigation to the adjacent fields of $3CaO.2SiO_2(C_3S_2)$ and of $CaO.SiO_2(CS)$ as well as to the fields of the calcium borates, $3CaO.B_2O_3(C_3B)$, $2CaO.B_2O_3(C_2B)$, and $CaO.B_2O_3$-(CB). This more complete study is of significance not only with respect to cement chemistry but in relation to the constitution of enamels and ceramic glazes. The portion of the system lower in lime and higher in silica and boric oxide has a bearing upon the constitution of borosilicate glasses.

The following binary or pseudo-binary systems belonging to the ternary system have been completely or partially worked out by heating curves and quenchings upon 114 different compositions: CB–CS, CB–C_3S_2, CB–C_2S, CB–C_3S, C_2B–C_2S, C_2B–C_3S, and C_3B–C_2S. Melts containing 44.3 per cent. C_2B, 55.7 per cent. C_3S, or 58.0 per cent. C_3B, 42.0 per cent. C_2S form the ternary compound $5CaO.B_2O_3.SiO_2$-(C_5BS). Determinations of the primary phases and melting points of the various compositions have indicated approximately the boundary curves of the fields of C_2S, C_3B, C_2B and partially the limits of the fields of CS, C_5BS, and CB.

It has been found that dicalcium silicate takes up considerable quantities of boric oxide (added as monocalcium borate) in solid solution by which the alpha-beta inversion temperature is lowered as much as 130°.

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Mr. Eugene E. Nice, Philadelphia, Pa.
Dr. Wilbur M. Stine, Upper Darby, Pa.
Mr. H. F. Taylor, New York City.

LIBRARY NOTES.

RECENT ADDITIONS.

Achema Ausstellungs-Führer. 1930–1934.
Achema-Jahrbuch. Jahrgang 1926–1934. 1927–1934.
American Pharmaceutical Association. Year Book. Volumes 18–21, 1929–1932. 1931–1934.
BERZELIUS, JÖNS JACOB. Autobiographical Notes. Published by The Royal Swedish Academy of Sciences through H. G. Söderbaum. Translated from the Swedish by Olof Larsell. 1934.
British Journal Photographic Almanac. 1934.
CORLISS, MARY. Life and Work of George H. Corliss. 1930.
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ELLIS, CARLETON. The Chemistry of Petroleum Derivatives. 1934.
EMELÉUS, K. G. The Conduction of Electricity through Gases. No date.
EUCKEN, A., UND K. WOLF. Hand-und Jahrbuch der chemischen Physik. Band 9, Abschnitt 2. 1934.
GREENE, ARTHUR M., JR. Elements of Hydraulic Power Generation. 1934.
Jahresbericht über die Leistungen der chemischen Technologie für das Jahr 1933. 79 Jahrgang, 2 Abteilung: organischer Teil. 1934.
Journal of Biological Chemistry. Index: Volumes 76–100, 1928–1933. 1934.

KRUYT, H. R. Les Colloides. Manuel de Chimie Colloidale Traduit d'après l'Edition Anglaise par J. Du Plessis de Grenédan. 1933.

London. Metropolitan Water Board. Thirtieth Annual Report for the Year Ended 31st March, 1933.

MILLS, JOHN. Signals and Speech in Electrical Communication. 1934.

MINERVA. Jahrbuch der gelehrten Welt. Einunddreissigster Jahrgang, 1934. Zweite Abteilung: Universitäten und Fachhochschulen.

Society of Dyers and Colourists. Jubilee Issue of the Journal of the Society of Dyers and Colourists. 1884–1934.

Technologie der Textilfasern. Herausgegeben von R. O. Herzog. VIII Band, 3 Teil, A. 1934.

WOOD, ROBERT W. Physical Optics. Third edition. 1934.

World Power Conference. Sectional Meeting. Scandinavia. 1933. Transactions. Volumes 5–6. 1934.

BOOK REVIEWS.

ELEMENTS OF HYDRAULIC POWER GENERATION, by Arthur M. Greene, Jr., Dean School of Engineering, Princeton University; Sometime Professor of Mechanical Engineering, Rensselaer Polytechnic Institute; Junior Dean, School of Engineering, University of Missouri; Instructor, University of Pennsylvania. 58 pages, illustrations, 15 × 23 cms. New York, John Wiley & Sons, Inc., 1934. Price $1.00.

Only a short while ago, Professor Greene's recent introductory treatise on the "Elements of Power Generation" was noticed in these pages. In that ample volume, steam generators and their auxiliaries occupied a dominant position, other methods of power generation, although by no means neglected, receiving secondary consideration in point of amount of space devoted to these other methods and the devices which are employed in their application. The present volume is designed as a supplement to the previous work in which the consideration of hydraulic power machinery and installations is greatly amplified.

The presentation of the subject-matter harmonizes with that employed in the general work; the same concise descriptions interspersed with illuminating related theoretical considerations, the same insistance on accurate terminology by underscoring all important terms which apply to this art, and the generous number of superior line cuts and half-tones. The illustrations are of the most recent types of generators and up-to-date installations.

The typography is unusual, and reminiscent of a printers' strike which occurred in this country some twenty years ago. The entire text is printed from plates reproduced from typewritten copy. That method of producing printed matter has obvious advantages in eliminating typographical error and possibly in time of production.

The volume should make a strong appeal to all readers of engineering literature who must have accurate information on the elements of modern hydraulic power practice.

L. E. P.

NATIONAL ADVISORY COMMITTEE FOR AERONAUTICS

Report No. 484, A Flight Investigation of the Effect of Mass Distribution and Control Setting on the Spinning of the XN2Y-1 Airplane, by N. F. Scudder, 10 pages, tables, 23 × 29 cms. Washington, Superintendent of Documents, 1934. Price five cents.

The investigation of the effect of mass distribution on the spinning of airplanes initiated with the tests on the NY-1 airplane has been continued by tests on another airplane in order to increase the scope of the information and to observe particularly the behavior of an airplane that shows considerable change in sideslip angle for its various conditions of spinning.

The XN2Y-1 naval training biplane was used for the present tests in which changes of ballast along the longitudinal and lateral axes and changes of aileron, stabilizer, and elevator settings were made. The effects of these changes on the steady spin were measured in flight.

The effects of varied mass distribution and control setting were found to be in agreement with the results found with the NY-1 airplane except for the cases in which changes in sideslip occurred with the changes in the other parameters of the spin. Satisfactory qualitative agreement between these tests and the theory developed in the analysis of the NY-1 spin, extended to include the effects of sideslip on equilibrium, was obtained when reasonable assumptions as to the aerodynamic properties of the airplane were made.

The effect of a large amount of ballast placed on the wing struts (not tested with NY-1) was to increase the angle of attack, rate of rotation, and inward sideslip. These effects were in agreement with the theory developed for the NY-1 airplane. No dangerous features of the recovery developed during the tests.

R.

MAGNETISM, by Edmund C. Stoner, Ph.D., Reader in Physics at the University of Leeds, Research Fellow of Emmanuel College, Cambridge, with a General Preface by O. W. Richardson, F.R.S., Yarrow Research Professor of the Royal Society, Nobel Laureate in Physics, 1928, 117 pages, illustrations, 11 × 17 cms. New York, E. P. Dutton and Company, Inc. Price $1.10.

A member of a series of Monographs on Physical Subjects, this pocket-size volume of Magnetism is written primarily for those to whom the subject is not new. The author has confined his attentions to those aspects of magnetism principally concerned with the magnetic properties of materials. It is hoped that the book will be useful as a general survey and as an introduction to those who wish to carry out research on the subject. Materials are classified according to whether they exhibit diamagnetism, paramagnetism or ferromagnetism. The discussion on each type of magnetism includes a careful consideration of existent theories and the experimental results reported by various investigators. The subject of ferromagnetism receives a more detailed treatment in view of its extensive practical utilization. Two chapters covering somewhat more fundamental topics also are included. These are: Magnetic Properties of Atoms and Magnetic Properties of the Elements. The volume ends with a note of reference to books and articles furnishing more elaborate treatment on the subject of Magnetism.

T. K. CLEVELAND.

ELEMENTARY QUALITATIVE ANALYSIS FOR COLLEGE STUDENTS, by J. H. Reedy, Associate Professor of Chemistry in the University of Illinois, 2d edition, 163 pages, tables, 14 × 21 cms. New York and London, McGraw-Hill Book Company, Inc., 1932. Price $1.50.

The second edition of this manual of elementary qualitative analysis for college students retains the general plan and arrangement of the first. The changes consist mainly in the inclusion of new procedures such as the use of perchloric acid as a rapid and safe solvent for the sulfides of the Tin Subgroup; the precipitation of potassium by a concentrated solution of zinc perchlorate; the precipitation of sodium by the new zinc-uranyl acetate reagent; and, a new scheme for detecting anions.

Essentially the book represents an attempt to put in printed form the system of laboratory and quiz room instruction that has been developed during recent years in the elementary courses in Qualitative Analysis at the University of Illinois. An effort has been made to impress upon the student the realization that every qualitative procedure is based upon some fundamental reaction or particular characteristic of the substance involved. For this reason, theoretical discussions to explain results are given as notes, placed as close to the experimental directions as possible. The procedures in this manual have been chosen with a view to their reliability in the hands of elementary students.

T. K. CLEVELAND.

ELEMENTARY LABORATORY EXPERIMENTS IN ORGANIC CHEMISTRY, by Roger Adams, Professor of Organic Chemistry in the University of Illinois, and John R. Johnson, Professor of Organic Chemistry in Cornell University, 363 pages, illustrations, 14.5 × 22.5 cms. New York, The Macmillan Company, 1933. Price $1.90.

The structure of this manual reflects accurately the extensive experiences of the authors as teachers of Organic Chemistry. Its thoroughness and its method of approach to a somewhat more than average empirical study places this particular collection of laboratory experiments in Organic Chemistry well in the forefront of manuals of this type. Although designed for beginning students whether specializing in the subject or taken in preparation for medicine, agriculture, home economics, biology or related fields, it covers quite thoroughly the principles of organic synthesis technique.

The manual is in two sections, the first of which comprises a series of experiments on laboratory operations. These include such manipulations as melting point determinations, crystallization, fractional and steam distillation, extraction and the qualitative as well as quantitative determination of elements. The second section deals with the preparation and reactions of typical organic compounds. Directions are furnished for the preparation of over fifty compounds. The choice of compounds was largely influenced by the desire to acquaint the student with the better-known typical reactions in organic syntheses. Not the least useful part of the manual is the Appendix. Herein is found a statement of the approximate time required for each experiment, the physical properties of some organic compounds not listed in the ordinary handbooks, properties of some aqueous solutions and a complete list of the materials and reagents required for the experiments.

T. K. CLEVELAND.

SEEING AND HUMAN WELFARE, by Matthew Luckiesh, D.Sc., Director, Lighting Research Laboratory, General Electric Company, Nela Park, Cleveland, 193 pages, plates, 14 × 21 cms. Baltimore, The Williams & Wilkins Company, 1934. Price $2.50.

A writer and director of research has once again laid aside his vocabulary of technical phraseology and added to an already long list of writings, that of "Seeing and Human Welfare." In living up to the purpose implied by the title, that of improving human welfare, the author has adopted the language of the educated layman and not that of the trained specialist in science. Most readers will concur with Dr. Luckiesh in his apparent conviction that the importance of preserving one's eyesight cannot be stressed too highly nor reiterated too often. Sight, that "sense" upon which a human being relies the most, is shown to exert a far-reaching influence upon racial welfare. Naturally, any defections in the process of seeing will be reflected by a proportionately larger number of injurious results.

Speaking generally the author cites many abuses that are sustained by the human seeing mechanism. Perhaps the reader was aware of most of these, but never considered them seriously, having heard of them much in the manner that folk-lore and superstitions are communicated. In this book, there is no equivocation; the author's statements are supported by scientifically controlled investigations.

However, the book does not stop there. Data giving the correct degree of illumination for each visual task also is furnished. Facts little known in a popular way, are presented as a basis for formulating all "seeing" operations. The natural focal point for the human eyes offers a logical starting point. If printed matter is held at the focal distance from the eyes, then one source of optic muscular fatigue is eliminated. To prevent other varieties of eye strain, the type on the printed page must not fall below a certain minimum. There is the matter of contrasts to be considered. The ease with which an object is recognized visually depends to a large extent upon how readily it may be distinguished from its background.

Manufacturers are rapidly learning the economic and psychological importance of correct illumination in order to render seeing as effortless as possible for their workers. Perhaps such utilization is least altruistic of all efforts that might be put forth to improve seeing and hence human welfare. The book makes all this information now available to the architect, the teacher, directors of institutions and best of all, the home-owner.

T. K. CLEVELAND.

PUBLICATIONS RECEIVED.

Loud Speakers, Theory, Performance, Testing and Design, by N. W. McLachlan. 399 pages, illustrations, 17 × 24 cms. Oxford, Clarendon Press, 1934. Price $13.50.

The Electronic Structure and Properties of Matter, vol. 1 of A Comprehensive Treatise of Atomic and Molecular Structure, by C. H. Douglas Clark, 373 pages, tables, 14.5 × 22.5 cms. New York, John Wiley and Sons, Inc., 1934. Price $5.50.

National Research Council, Transactions of the American Geophysical Union,

Fifteenth Annual Meeting, Parts I and II. 17 × 25 cms. Washington, National Research Council, 1934.

United States Bureau of Mines, Mineral Resources of the United States, 1931, Part I—Metals, 710 pages, tables, 15 × 23.5 cms. Washington, Government Printing Office, 1934. Price $1.50.

Bell Telephone Laboratories, Monographs, B-788, Line Filter for Program System, by A. W. Clement, 10 pages, illustrations, 15 × 23 cms. B-791, Propagation of High-Frequency Currents in Ground Return Circuits, by W. H. Wise, 6 pages, 15 × 23 cms. B–794, The Effect of Background Noise in Shared Channel Broadcasting, by C. B. Aiken, 18 pages, illustrations, 15 × 23 cms. New York, Bell Laboratories, 1934.

Ontario Department of Mines, Forty-Second Annual Report, vol. 42, parts 4, 5 and 6, 1933, 16.5 × 25 cms. Toronto, King's Printer, 1934.

National Advisory Committee for Aeronautics, Technical Notes No. 502, Aerodynamic Investigation of a Cup Anemometer, by John D. Hubbard and George P. Brescoll, 24 pages, tables, plates, 20 × 26 cms. Washington, Committee, 1934. No. 503, Measurement of Altitude in Blind Flying, by W. G. Brombacher, 33 pages, illustrations, 20 × 26 cms. Washington, Committee, 1934.

Jöns Jacob Berzelius, Autobiographical Notes, published by the Royal Swedish Academy of Sciences through H. G. Soderbaum, translated from the Swedish by Olof Larsell, 194 pages, plates, 14×21 cms. Baltimore, Williams and Wilkins Company, 1934. Price $2.50.

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CURRENT TOPICS.

Interesting Uses for Tin.—(*Chemistry & Industry*, Vol. 53, p. 732.) A summary of the first general report issued by the International Tin Research and Development Council describes a great variety of purposes for which tin is employed. Some of the more interesting applications are mentioned herewith.

In the method of preparing alloys from powdered metals, bronze may be produced by intimately mixing in suitable proportions finely divided tin and copper which are then heated under reducing conditions. If graphite is mixed with the metals the result is a bearing-metal of the oil-less type.

A lead-tin-cadmium alloy for cable sheathing shows a higher resistance to soil corrosion. This alloy also is being tried for water pipes for use in certain districts.

Tin-lined lead pipes are at present being manufactured but stronger, lighter pipes and suitable methods of jointing are necessary in order to compete with other materials.

Tin accumulators now exist. These cells contain electrodes of tin and lead peroxide respectively and the electrolyte is dilute sulphuric acid. The whole of the cell is filled with porous acid-resisting ceramic material in which the acid is absorbed, so that it is in effect a dry battery. As no gas is evolved during discharge, the cell may be sealed and used in a horizontal position. It is smaller in weight compared to a lead accumulator of similar capacity.

C.

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